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THE PHILOSOPHICAL CONCEPTS IN MODERN PHYSICS.*

MATHEMATICAL MODELS IN MODERN PHYSICS.

BY

E. U. CONDON, Ph.D.,

Associate Director, Westinghouse Research Laboratories, East Pittsburgh, Pa.

"All is fair in love and war" and, I might add, in theoretical physics. As in those activities painful adjustments are often necessary. Until a few years ago physics has been so definitely tied to Newtonian mechanics that, for many persons, progress in physics has come to mean understanding of phenomena within that frame. So much has been accomplished within that frame that it is naturally tempting to believe that mathematical physics should be thus restricted and that a phenomenon is not "explained" unless it is fully described in terms of classical mechanics.

Recent trends have emphasized the movement away from this restricted point of view by development of a non-Newtonian scheme of quantum dynamics within the field of dynamics itself. To be sure it had previously been recognized that classical dynamics alone will not give us everything. I refer especially to the inclusion of statistical ideas to explain the second law of thermodynamics, and to the difficulties of

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combining mechanical models of the electromagnetic field with the requirements of the Lorentz transformation. But in quantum mechanics we find the field of dynamics itself being invaded and revised, instead of simply being supplemented.

These trends raise the general questions: what *is* fair in theoretical physics? Shall we be satisfied with non-Newtonian explanations? What constitutes a satisfactory solution of a problem in theoretical physics?

It is not my desire to relate these questions to any profound presentation of technical philosophy. I shall simply try to expound a position which seems to me satisfactory enough to live in from day to day while doing one's work.

I accept the fact of man's intellectual curiosity, motivated directly or indirectly by the biological necessity of adaptation to the environment—of getting along in the world. John Dewey has said:[1] "Reflection is an indirect response to the environment, and the element of indirection can itself become great and very complicated. But it has its origin in biological adaptive behavior and the ultimate function of its cognitive aspect is a prospective control of the conditions of the environment. The function of intelligence is therefore not that of copying the objects of the environment, but rather of taking account of the way in which more effective and more profitable relations with these objects may be established in the future."

This purposive curiosity leads man to gather information about the environment in many ways, both natural and highly artificial and to reflect on it. He endeavors to build up some logical scheme into which his observations will fit for convenience in remembering them and transmitting them to others. Characteristic of modern times is the large scale on which activity of this kind is being done and its socialization. Formerly the spare time activity of a few gentlemen of culture, scientific study is now principally in the hands of professionals supported socially for full-time occupation at such work. Also characteristic of the times is the high degree of specialization which has resulted. Among the specialists we find physicists and among them more specialized yet a special breed of beings called mathematical physicists.

[1] "Philosophy and Civilization," Minton, Balch, 1931, p. 30.

What are they doing and what is their function? Everybody knows what they do—they study carefully the results obtained by experimentalists and rewrite that work in papers which are so mathematical that they find it hard to read them themselves. What for? What values do they contribute? Or, as deKruif would ask, why keep them alive?

The answer is plain enough. *I take it to be the object of physics so to organize past experience and so to direct the acquisition of new experience that ultimately it will be possible to predict the outcome of any proposed experiment which is capable of being carried out—and to make the prediction in less time than it would take actually to carry out the proposed experiment.* When this shall have been done I will say that man has a complete understanding of his physical environment. Others may ask more, with this I am satisfied.

I lay great stress on the requirement that we be able to make the prediction in less time than it takes to try the experiment. The requirement is not strictly formulated: in some cases a loss of time could be tolerated to effect a great saving in the expense of performing an experiment.

The accomplishment of this goal obviously calls for a vast amount of experimental work but it also calls for a vast amount of organization of the results of experiment into concise forms which permit of ready application of the knowledge contained in them to new situations. Every experimental observation is a particular instance and is without value unless we may safely generalize from it. A certain amount of the generalizing is done by the experimentalist who makes sure of the definiteness of his conditions with control experiments and criteria of reproducibility. But usually that level of generalization covers only the critical evaluation of the data obtained in a particular experimental set-up or class of set-ups. After the experimentalist has provided good data in this sense someone must study its relation to the general body of knowledge previously known in order to fit it in the most consistent and concise way possible. There must be someone who is constantly watching the growth of that body of knowledge to see that no dangerous holes or weak places are left in it and above all to see that we are really getting a body that is functional: it must meet the test of "prospective

control of the conditions of the environment." The theoretical physicist is the architect of that body of knowledge which the experimentalist is building for us.

Who will say what style of architecture he should employ?

The given data are usually in the form of particular quantitative statements of relation between numbers obtained in experimental observations. From these particulars one must try to infer a general relation structure embracing the expected result of all similar observations which might have been made but were not, and suggesting an expected result of other experiments which might be made in the future. Pure mathematics is the science of abstract relation structures, of the relationships which are capable of being stated in exact and quantitative form. It follows that pure mathematics is the chief tool of the theoretical physicist—so much so that he is often referred to as a mathematical physicist, a name that characterizes him by his tools rather than his function.

From this point of view it is clear that we have no a priori right to prescribe that only certain parts of the general theory of relations are to be used in coordinating experimental facts. Historically the methods of infinitesimal calculus as used more particularly in Newtonian dynamics have played the greatest role in the science of physics up to this century. So great has been the progress by these methods alone that it is no wonder that a school of thought grew up which identified progress in theoretical physics with the obtaining of results in this way. Naturally when certain leaders of theoretical physics found it convenient to employ more general mathematical methods followers of this school were greatly disturbed.

They were disturbed partly because they were more or less consciously committed to the restricted point of view which requires a physical theory to be Newtonian in form. Also, of course, they were disturbed because we all have our limitations and it really is hard to have to learn whole new branches of mathematics in order to extend one's "prospective control." I think this latter reason has, naturally enough, carried the most real weight, with the former reason being offered up as the rationalization because it sounds better.

If we look back over the history of modern theoretical physics we will see that the first kind of mathematical structure beyond the calculus of Newtonian mechanics to be used was the theory of probability. With the founding of the kinetic theory of gases by Maxwell and Clausius in the middle of the nineteenth century, the statistical method entered physics. Regularly predictable phenomena of nature were correlated with statistical averages of undetermined and basically chaotic features of the model. The method was non-Newtonian in that it introduced supplementary statistical principles—to be sure the model, which was considered statistically, itself was governed by Newtonian mechanics.

This development never seems to have caused much of a stir philosophically. Yet it represents a radical extension of the method of mathematical physics beyond the Newtonian scheme. Why was there no stir? Probably because the model of the gas did obey Newtonian mechanics so the statistical features could be regarded as temporary and provisional stages of development to be removed by the time the theoretical edifice is completed.

But as time went on the statistical features became more and more essential to our understanding of thermal phenomena. The work of Boltzmann and Gibbs gave us a clear account of the fundamental meaning of the second law of thermodynamics in a form in which that great principle was essentially connected with the theory of probability. With the acceptance of their work, which was general a quarter century ago, it should have been clear that a wider base than afforded by Newtonian mechanics was being used for the construction of theoretical physics.

Paralleling these developments we see a great deal of effort being expended to provide a Newtonian mechanical foundation for the laws of the electromagnetic field as formulated by Maxwell. Since 1905 we have realized, however, that progress in this direction could not be made. This means that the laws of the electromagnetic field have to be accepted as a set of empirical equations, self-consistent, not inconsistent with mechanical principles, but also not explained in terms of a detailed mechanical model of the medium.

The middle period of development of quantum theory, in the years from 1912 to 1925 was characterized by the use of dynamics in the traditional mathematical form to which some additional ideas were attached. This was clearly recognized as a provisional stage by Bohr and others who played an important part in those developments—a partially successful scheme serving only to point the way to a more complete form of theory.

This more complete form came, as everyone knows, in the years 1925–27 with the work of deBroglie, Heisenberg, Schrodinger, Born and Dirac. In the decade which has followed there has been a flourishing growth of the theory of atomic structure. By means of the principles of quantum mechanics all of the known phenomena of atomic and molecular structure have found qualitative, and in many cases quantitative, interpretation. In the last three or four years this work has slowed down somewhat partly because all the relatively easy problems are done, partly because this work is outshone by the vigorous growth of experimental nuclear physics. It is interesting to note that definite theories of nuclear structure are so new that they have never existed in any other form than as quantum mechanical theories.

The new and unusual mathematical forms employed by the developments have caused a great stir in our physical thinking. The new methods have called for a complete revision of our basic ideas in dynamics in such a way as to leave no doubt about the fact that Newtonian methods are completely outgrown. Whether this is regarded as a satisfactory state of affairs depends, of course, on whom we are trying to satisfy. Naturally if one believes that theoretical physics must ultimately be constructed wholly by Newtonian methods he will feel that the progress of the past decade is progress only in the sense that we have a scheme which gives many empirical correlations but whose deeper meaning has not yet been sensed. However, if he does not hold to such a more restricted view of the form of the ultimate development but merely requires internal consistency and agreement with observation then most of the developments of the past decade are indeed highly satisfactory. I have tried to indicate in the

first part of these remarks a point of view according to which this second attitude seems to be tenable.

By that I mean that we have a reasonable view of science according to which we can regard the developments of recent years as a possible ultimate theory without feeling dissatisfied. Of course I do not mean to insist that the present form of quantum mechanics *is* ultimate nor that possible future developments which go beyond present ideas would be unsatisfactory or superfluous from the point of view taken here. Personally I believe that the theory is not ultimate, that the theory calls out for development in a direction which makes the Pauli Exclusion Principle be an inevitable rather than merely a possible consequence of its mathematical structure. But I also believe that these developments will come not by seeking for classical mechanical models of the universe but by more extensive exploration of the possible forms of mathematical model which are capable of coordinating all our past and future experience of the physical world.

In conclusion I want to make it clear that I am not attempting to act as spokesman for the scientific movement. The views expressed seem reasonable to me and I hope they will seem so to others but no one is asked to accept them on any other basis than their appeal to his own reason.

Weather Bureau Helps Mankind to Avoid Vagaries of Weather.—What a national weather service can do in minimizing the unhappy and sometimes tragic effect of the weather on mankind was shown in the last fiscal year, according to W. R. GREGG, Chief of the Weather Bureau, in his annual report to the Secretary of Agriculture. Although dependable weather forecasts for more than a few days in advance are not yet possible, day-by-day summaries of conditions throughout the country, coupled with short time forecasts, gave Government agencies and the American Red Cross information which enabled them to reduce to the minimum the suffering and loss caused by the drought of 1936. Advance news of high river crests kept down the death toll and economic losses from the Ohio and Mississippi floods. Without the Bureau's timely warnings of extreme low temperatures, practically the entire citrus fruit industry of Southern California would have been wiped out by the record-breaking freeze last January. Orchard heaters, fired when frost warnings were received, saved the fruit in many places. The crop in groves not provided with heaters was almost a complete loss. In planned aerial flights the weather plays an important part. Against the few accidents that attracted nation wide attention, hundreds of flights were postponed or canceled because of warnings of bad weather. Other life and property losses that would have reached the public ear were prevented—and so never noticed—by holding ships in port when the Bureau's storm warnings were hoisted; by concentrating firefighting forces when the weather favored destructive fires in the national forests. The Bureau has strengthened its service by more frequent reports from a closer network of stations and by the use of better instruments.

R. H. O.

OPERATIONAL REASONING, REALITY, AND QUANTUM MECHANICS.

BY

EDWIN C. KEMBLE, Ph.D.,

Professor of Physics, Harvard University.

As one surveys the development of physical science since the beginning of the century two things stand out. The first of these is the extraordinary rate at which the science has developed, whatever the criterion used to judge its advancement. The second striking fact is the growth of a smoke screen of unintelligibility about the work of the physicist that at times bids fair to cut off completely his line of communication with the rest of the intellectual world. The seriousness of this breakdown of communications has been emphasized by Dingle in a recent article [1] so that I need not dwell on it here.

The difficulty experienced by a physicist in explaining his work to non-professionals is shared in part by those engaged in every other activity which employs a highly developed technique. It has become much more serious, however, on account of the tendency toward abstractness in the formulation of physical theory. This tendency toward abstraction has been accompanied by much confusion of mind among physicists themselves as to the proper meaning of the terms used in stating theory and by a new interest in the philosophic criticism of the fundamental concepts of physics. It has been aggravated by the inevitable inclination of popular writers to play up the paradoxical elements of relativity theory and quantum theory for a mystery-loving public.

There is, I believe, no possibility or reversing the trend towards abstraction in theoretical physics. That trend has its origin in nature itself and in the will to power over nature on the part of the research worker. I do not believe that it

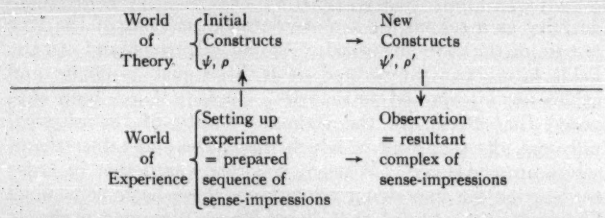
[1] H. Dingle, "Knowledge without Understanding," *Atlantic Monthly*, July, 1937, p. 116.

is a matter of taste among theory builders. As physicists we can only face the situation and seek a better understanding of what we are doing in the light of which our primary results can be reported back to the rest of the world with maximum clarity. Happily the ground work for such a clear understanding was laid in the 1890's by the Viennese physicist and philosopher, Ernst Mach. The recent progress of physics adds little that is new to philosophic thought, but it underscores the brilliant analysis of Mach and so exerts a strong influence on the present trend of philosophic opinion.

Consider, then, the objective and technique of physical science. I take its purpose to be the securing of maximum control over our physical environment. The procedure is one of experiment and analysis designed to reveal the rules telling us what always happens under certain circumstances. In a world in which the circumstances are always different, unless we are repeating an experiment, it is important that we find flexible rules adaptible to varying conditions. Hence we seek to pass whenever possible from particular rules to more general ones including the others as special cases. Thus the ideal toward which the physicist strives is a universal rule or formula into which information defining any physical situation can be inserted and from which it is possible to derive all the information regarding the future which is inherent in the physical situation.

In the process of developing such rules, or theories, we have been led to invent abstractions, or constructs, not directly items of experience, such as electric and magnetic fields, atoms, electrons, wave functions and dynamical variable matrices. These constructs are the material of our thought and the symbols which appear in the mathematical formulas which express our rules. As Margenau has recently emphasized,[2] the fundamental procedure in theoretical physics is to translate the given data for a physical situation into the language of our constructs, apply the laws of physics formulated in terms of construct symbols and then reinterpret our conclusions in terms of experience. The process is illustrated in the accompanying diagram.

[2] Henry Margenau, "Philosophy of Science," 2, p. 48 and p. 164, 1935.



Here the lower horizontal arrow denotes the process of letting the experiment run, while the upper one indicates the application of mathematical rules characteristic of the world of constructs.

No one can quarrel with the physicist for the introduction of constructs since everyone must recognize the fact that in the last analysis we apprehend directly only our own sensations. Thus the world of space, time, things, and people in which we conceive ourselves to live is really an invention or construct which every human being makes instinctively in order to create some order in the moving panorama of experience. Mach very beautifully explains that even the ego itself is a construct and not a primary datum. As I see it, all thought is a manipulation of constructs which we ordinarily correlate with experience by instinct.

The difficulty is that as our theories in physics have become more powerful new constructs have been needed which are more remote from experience than the old ones. Hence the correlation between the world of our sense impressions and the world of constructs has ceased to be instinctive. The rules governing this hitherto unregarded process must be studied and written down. This is what I mean by the tendency to abstraction. From the standpoint of common sense there exist two universes, or worlds, viz., an internal world of perception, thought, and emotion and an external world of space, time, and matter which is assumed to impinge on the internal world in experience. The external world, although in fact a construction, is deemed as real as the internal world. The physicist in developing his theories is actually carrying farther the process of invention begun in infancy and therefore has been accustomed to regard his

activity as a search for a more detailed account of the true nature of the external world. Atoms, electrons, and electric fields have been interpreted as realities just as chairs and tables are interpreted as realities. In fact it has been supposed that these are the ultimate reality of the physical universe, the true stuff of which the chairs and tables themselves are composed. When it was discovered that in order to describe our experience adequately these basic constructs had to be so modified that it no longer remained a simple matter to interpret them as pictures of an external real world many mental adjustments had to be made and numerous apparent paradoxes arose to disturb the sleep of the conscientious physicist.

My first thesis today is that most of the paradoxes are not really problems for the physicist although they may be of considerable interest to the professional or amateur philosopher. This is because they do not really involve any conflict between theory and experiment. They have to do with elements in the theory which do not look right from the standpoint of common sense and mark the divergence between new constructs and old. But the concern of physics is the correlation of experiences. Its domain is the domain of the experimentally verifiable. Therefore the job of the physicist is to describe the experimental facts in his domain as accurately and simply as possible, using any effective procedure without regard to such *a priori* restrictions on his tools as common sense may seek to impose. His one semi-philosophical concern must be to clarify fully the procedure whereby he passes from his sense-perceptions to the world of his constructs and back again.

In so doing he will of necessity adopt the method of operational reasoning recently advocated by Bridgman and closely related to the positivism of Mach and the pragmatism of the American philosopher and scientist, Charles S. Peirce. The essence of this practical method is the demand that merely verbal questions be ruled out of all scientific discussion. Unless this is done thought and communication become farcical. To clarify our ideas it proposes to define all concepts used in discussion by relating them directly to experience, rather than by assuming that they are entities with certain

preassigned properties.[3] Thus Bridgman would say that a table is defined by the rules for recognizing one. Mach would say that a table is but the totality of the sense-impressions which we associate with it. Peirce would say that the only distinction between a table and any other object is in the sensible effects which it produces. You will see at once that these are but different ways of recognizing the fact that the table belongs to the world of constructs and has no meaning or importance aside from its relation to the primary world of experience. Similarly the operationalist is accustomed to say that the physical properties of various objects are defined by the method of measuring them. This implies that the phrase "to make a temperature measurement of the air in this room" has a definite operational meaning. The number obtained by such a measurement also has definite operational meaning. On the other hand one can give no such meaning to the term "temperature" as applied to a room not under observation except as the word may stand for a tool of calculation, or qualitative thought, ultimately intended to yield information regarding the results of future observations.

Without further general explanation let me show you the operational method in action.

Consider first the fundamental paradox of the quantum theory, viz., the fact that matter—and also radiation—acts as if it were both waves and corpuscles. How, says common sense, can electrons be two such obviously contradictory things? The first answer of the operationalist is that what electrons *are* is not the concern of physics. For him the word "electron" stands for a construct used in the correlation of experiences. Its meaning is defined by the type of mathematics with which it is associated, and by the manner in which we relate the mathematics with concrete sense-impressions. In other words we are concerned only with what we call the "sensible properties of electrons."

[3] The recent criticisms of the operational method by Margenau (l.c.) and by Lindsay ("Philosophy of Science," 4, 456 (1937)) are based on a very narrow interpretation of the writings of Bridgman, whereas I here adopt a broad interpretation which preserves the spirit of Bridgman's work and links it with that of Mach and Peirce.

Consider an experiment in which a beam of electrons passes from a hot filament through a vacuum tube and impinges on a fluorescent screen. This statement in construct language implies that a certain sequence of sense impressions has passed through the mind of the observer. A transverse magnetic field is applied and the deflection of the beam observed. This means that the observer experiences a pattern of sense-impressions which we call "closing a switch" followed by another which we call "looking through an observing microscope." From what he sees in the microscope the experimenter evaluates a number which he calls the deflection of the beam and which he combines with other numbers giving the length of the path, the strength of the field, etc., in order to check observation and theory. If a theoretical formula is used based on the mathematical apparatus of the classical mechanics and electrodynamics, if the experiment is repeated many times with a variety of initial conditions, and if the results check the calculations to a reasonable extent—as they will in practice—we can say that the electron has passed the initial tests for admission to the corpuscle class. The electron diffraction experiments of Davisson and Germer, however, demand a different type of theory in which the mathematics of classical mechanics is replaced by an entirely different procedure and wave properties come into prominence. In order to describe adequately the rules governing all those experiences which we commonly relate to the properties of electrons it becomes necessary to use the complete mathematical apparatus of quantum mechanics. This apparatus is part of the definition of the electron construct as we now use the term. It contains elements which relate it to the classical corpuscle theory and to the classical wave theory, but it is neither one nor the other. The situation seems paradoxical only in so far as we obstinately refuse to admit the validity of constructs with unfamiliar properties.

Permit me now to turn to a brief consideration of the way in which the mathematical machinery of the quantum theory is related to experience.

In classical mechanics the construct called "the momentary state" of a system is given by an enumeration of the values of its coordinates and of their velocities. In quantum

theory we replace this set of numbers by a function ψ of the coördinates alone which varies in time so long as the system is undisturbed according to the Schrödinger equation

$$H\psi = -\frac{h}{2\pi i}\frac{\partial \psi}{\partial t}. \qquad (A)$$

Here H is an operator which—according to one's point of view—defines, or is defined by, the nature of the system under consideration. Eq. (A) describes a deterministic law of evolution characteristic of the new world of constructs. ψ is given physical meaning by means of the fundamental hypothesis that $|\psi|^2 d\tau$ denotes the probability that a measurement of configuration made on the system in the given state will give a result which belongs to the small range of coördinate values indicated by $d\tau$. There is also a definite, but more complicated, procedure for evaluating the probabilities of other dynamical variables, such as energy and momentum, by appropriate manipulation of the ψ-function.

As Professor Slater pointed out years ago, the probability postulate can be given operational meaning only if any measurement made to test it is repeated many times under identical conditions. On account of the disturbance of the system by a measurement this means that we must many times bring the system back to its initial state in order to repeat the measurement properly or else make a statistical measurement of a large Gibbsian assembly, or ensemble, of similarly prepared, identical, independent systems. In other words quantum mechanics is a form of statistical mechanics properly comparable to the classical statistical mechanics of Gibbs. As I see it, the wave function ψ has no meaning for individual systems that is not implicit in the statement that it fixes the distribution of results for any type of statistical measurement when made on a sufficiently large assembly of systems in the state correlated with ψ. This statement confronts us with the essential indeterminism characteristic of the world of experience and incorporated into quantum theory as a basic postulate.

The fundamental role of assemblies in quantum theory is disturbing at first to one accustomed to regarding the laws of physics as categorical imperatives that must be obeyed in

every instance. It becomes less disturbing when we remember the inevitable experimental error of all measurements and reflect that no experiment is informative in the scientific sense unless it is actually, or by implication, many times repeated in order to ascertain the variation of successive observations. Even one's acceptance of the butcher's single measurement of the weight of a roast is dependent on a multitude of past experiences which have shown that the variation in successive measurements with such an instrument is negligible for our purpose.

There has been a good deal of difference of opinion among physicists regarding the relation of quantum mechanical theory to determinism. The more orthodox opinion is that the success of quantum mechanics demonstrates the essentially indeterministic character of "nature." Some, however, have pointed to the deterministic equation (A) and have asserted that this equation means that undisturbed nature is deterministic and is given the appearance of indeterminism by the clumsiness of our methods of observation. However, the latter view makes sense only if one attributes to the world of constructs a reality superior to that of the world of experience which it is designed to explain. From the positivistic standpoint this is nonsense. Furthermore, as I hope to show, any attempt to interpret ψ-functions as faithful pictures of external reality rather than as tools of calculation leads to hopeless difficulty.

Much of the work of the quantum theorist can be based on the elementary construct which we call the wave function. A more complicated construct is needed, however, before the theory can be adequately fitted to experience. This is the density matrix.

In classical theory it was assumed that every system has at each moment a definite state even though that state may be unknown. Classical statistical mechanics, however, deals with constructs consisting of assemblies of systems involving a variety of classical states. So also in quantum mechanics we can imagine an assembly formed by compounding subassemblies with different wave functions. Such assemblies are usually referred to as *mixtures* in contradistinction to assemblies characterized by a single wave function. We call

the latter *pure state* assemblies. v. Neumann and Dirac have shown how it is possible to invent so-called "density" matrices which are the mathematical representatives of mixed assemblies in our construct world. The assemblies most worth talking about are those prepared according to rules sufficiently complete to insure the possibility of duplicating them. Every such assembly is assumed to have a definite corresponding density matrix ρ. The law of evolution which gives the variation of the density matrix in time and the rules for predicting the result of any statistical observation made on such a mixed assemblage are definitely worked out, but need not be stated here.

So much for the construct world of quantum mechanics and the transition from the world of constructs to the world of experience. We now come to the converse problem of passing from the world of experience to the world of constructs. A rule for doing this will answer the question "How is the ψ-function or the density matrix ρ representing the initial state of the system to be determined?" Our first comment on this question is to point out that the function ψ or matrix ρ appropriate to any particular physical situation has to be determined by subjective material in the mind of the observer. The physical situation itself is defined by the sense-impressions by which we recognize it. Thus we can say that the "state" of a physical system is fixed by our memory of its past history, or by the rules which we use in an attempt to prepare an assembly of identical systems "in the same state."

The actual process of correlating wave functions with sense-impressions began as a matter of guessing based on certain definite experimental facts. The familiar formulas of Einstein

$$E = h\nu, \qquad p = h/\lambda \tag{B}$$

are inventions which permit us to fix in part the nature of the wave functions appropriate to systems prepared so that in classical theory they would be expected to have definitely known energies and momenta. A logical approach to this question consistent with the above formulas is now possible, however.

By hypothesis the wave functions are so-to-speak "ruled by our sense-impressions." Hence, if the wave function appropriate to the state of a physical system at a certain moment is assumed to be known, any subsequent observation of that system must produce a discontinuous change in the function quite different from the changes incident to the law of evolution (A) for undisturbed systems. Experience shows, for example, that it is possible to measure the position of a particle, or the configuration of a more complicated system, in such a manner that a second observation made soon after the first one will yield the same result as the first within the range of experimental error. Therefore if our original knowledge of the system was such as to fix its momentum rather accurately and was correspondingly vague as to its position, the result of an accurate positional observation must be to substitute for an originally diffuse wave function a radically different one representing a compact wave packet in positional-coördinate-space. This well-known discontinuous change in the construct which we use to give mathematical form to our information is called the "reduction" of the wave function. The fresh information completely alters the procedure to be used in preparing an assembly of similar systems. It therefore alters the statistical experiment involved in checking any inference we may draw from the wave function ψ. It is clearly sensible that a new mathematical function should describe the results of a new type of experiment.

In this process of reducing the wave function we have the key to the method actually used in entering the world of constructs from the world of experience. Let me turn aside, however, from the main line of our inquiry to ask and answer a question which must fundamentally affect our attitude toward the ψ-functions. Recent developments in physics have emphasized the fact that every measurement, when interpreted in terms of an external real world involves an interaction between the system observed and an observing mechanism which forms a kind of chain connecting us with the system. The question now arises whether the reduction of the wave function is adequately accounted for as the result of the modification of the law (A) due to the interaction of the observing mechanism with the observed system. If we

could answer this question with a "Yes" we should go a long way toward reestablishing the old notion that the world of constructs which proves most effective in establishing correlations in the world of experience is actually a reflection of a real external world. Unfortunately we cannot give an affirmative answer to this basic question.

To fix our ideas let us consider a simple concrete example. Consider an experimental arrangement in which a stream of electrons originating in a small hot filament is accelerated by a known large potential difference and projected toward an opaque screen pierced by a small aperture. Since the forces acting between the electrons play a minor role, the stream constitutes in effect an assembly of independent identical systems similarly prepared. The assembly is strictly speaking of the mixture type, but the geometry of the problem and the fact that the energy dispersion for the stream is small so limit the wave functions which can enter the mixture that we can safely treat the assembly as a pure state for our present purpose. We shall consider the screen to be part of the observing mechanism. It divides the assembly of electrons into two parts consisting of those members which strike the screen and those which pass through the aperture. Treating the screen in theory as a fixed potential barrier we find that the initial wave function is also resolved by this piece of observing mechanism into two parts which can be correlated with the two subassemblies of electrons. In spite of this division we can continue to make observations on the assembly as a whole after its interaction with the screen. Or, we can concentrate our attention on one or the other of the subassemblies and experiment with it alone. In the former case we must use the complete wave function, as modified by interaction with the screen, in working out the theory. In the latter case we must renormalize the reflected or the transmitted portion of the wave function, as the case may be, and use that for the subassembly we choose to deal with. Thus the wave function is reduced by a *mental act* in which the observer transfers his attention from a primary assembly to a subassembly. Because of this fundamentally subjective element in the process of reducing the wave packet, which is characteristic of all measurements which have a predictive

aspect it seems to me that we *cannot* regard the wave functions which are the basic constructs of quantum theory as mathematical descriptions of any objective reality.[4] The essence of the idea of reality is that anything is real if it is independent of the way in which you think about it. This is not true of wave functions or density matrices. Hence I prefer to say that these mathematical tools of quantum mechanics specify the *subjective states* of the systems to which they apply and to consider that the *objective states* are undefined.

Let me now return for a moment to the question of the procedure to be used in passing from the world of experience to the world of constructs. It will be evident that a measurement which reduces the wave packet for an assembly initially in a pure state must cause a similar reduction or discontinuous modification in the density matrix of a mixed assembly. By appropriate manipulation and subdivision of any initial assembly we can recast the density matrix into one or the other of a great variety of forms. If the past history of a system, or assembly of systems, is such that, *whatever initial subjective state be assumed*, the records require that it shall now be in a definite pure or mixed subjective state we have the right to assign to it the corresponding mathematical construct. Thus the procedure for entering the world of constructs from the world of experience is defined. It is in principle more complicated than the reverse process, requiring as it does a theoretical analysis of the initial conditions, or of past history.

In conclusion permit me to summarize briefly. My fundamental point is that the puzzles of quantum theory have originated for the most part in the notion that its constructs are, or should be, direct reflections of the realities of the external world. I assert that the province of the physicist is not the study of an external world, but the study of a portion of the inner world of experience. Hence there is no reason why the constructs introduced need correspond to objective realities. An analysis of the relationship between the world of experience and the world of quantum-theory

[4] There are other arguments leading to the same conclusion. Cf., for example, Einstein, Podolsky, and Rosen, *Phys. Rev.*, **47**, 777 (1935).

constructs indicates that no such correspondence does in fact exist.

Please do not misunderstand me. Large scale experience fits with the common sense idea of an external world. I can no more deny the existence of such a world than you can. If you are not there, why am I talking to you? I admit I do not know exactly what I mean by saying that an objective external world exists. Perhaps some of my learned colleagues can give meaning to this assertion. However that may be, I am to date unable to escape the conviction that these words can be made to mean something that is fundamentally true and important. On the other hand it seems to me that the formulas of present day physics do not describe such an external world. In this respect I find myself coming to agreement with Einstein, Podolsky and Rosen, but I do not share their misgivings nor do I expect that the better physics of the future will remedy this defect.

New Fellowship Project to Accelerate Progress in Science.—A new kind of scientific expedition has been announced by the Westinghouse Electric and Manufacturing Company. In 1938 and 1939 the company will sponsor ten research physicists in exploring the fields of pure science. Five such explorers, to be known as Westinghouse Research Fellows, will be chosen the first year from the ranks of those having the equivalent of a doctor's degree in physics, chemical physics, or physical metallurgy and an additional five will be named later. The plan, as explained by Dr. L. W. CHUBB, director of the laboratories, provides for a continuing group of ten fellowships. The research work will be under the direction of DR. E. U. CONDON, formerly associate professor of physics at Princeton University. Though the Westinghouse fellowships are not designed to develop specific improvements or make concrete contributions in the practical application of electricity, Dr. Condon stated that: "Each widening of the horizon opens up new possibilities of technical advance although it is not usually clear at first what these possibilities may be. Past experience has shown that nearly all discoveries in pure science have sooner or later been of value. Westinghouse is supporting work in pure science in order that this work may be accelerated." Both Dr. Chubb and Dr. Condon agreed that the world's scientific advance would have been considerably more rapid in the past had the usual 20-year lag between a fundamental discovery and its application to industrial requirements been shortened by a closer link between research and industry. Improved electric lights, for example were made possible by the exact knowledge of what laws an electron obeys. These same laws were responsible for electronic tubes and control devices. It is planned to confine the Westinghouse work to fields for which the laboratories are well equipped, in particular, nuclear physics, electric conduction in gases, ferromagnetism, dielectrics, thermionics, semiconductors, elastic and plastic properties of metals and crystal structure.

R. H. O.

Neoprene Chloroprene Rubber.—At the annual meeting of the Society of Automotive Engineers in Detroit there was placed on display by the E. I. Du Pont de Nemours & Co., forty automobile applications of neoprene. They included carburetor hose, gas tank filler hose, vacuum gear shift hose, horn wire insulation, water pump seals, dust and grease and oil seals, oil pan gasket, radiator cap seals, and diaphragms for vacuum-operated spark, choke, and starter switch controls.

R. H. O.

ELECTRODYNAMICS OF PONDERABLE BODIES.

BY

J. C. SLATER, Ph.D.,

Professor of Physics, Massachusetts Institute of Technology.

A theoretical physicist in these days asks just one thing of his theories: if he uses them to calculate the outcome of an experiment, the theoretical prediction must agree, within limits, with the result of the experiment. He does not ordinarily argue about philosophical implications of his theory. Almost his only recent contribution to philosophy has been the operational idea, which is essentially only a different way of phrasing the statement I have just made, that the one and only thing to be done with a theory is to predict the outcome of an experiment. As a physicist, I find myself very well satisfied with this attitude. Questions about a theory which do not affect its ability to predict experimental results correctly seem to me quibbles about words, rather than anything more substantial, and I am quite content to leave such questions to those who derive some satisfaction from them.

The older quantum theory, up to about twelve years ago, was not a good theory. It was applied to numerous problems in atomic structure to which it gave wrong answers, and to others in which it was not able to give any answer at all. That fact distressed the theoretical physicists much more than the arguments about waves and particles, or about continuous and discontinuous quantities. Fortunately the labors of de Broglie, Schrödinger, Heisenberg, Born, and others, produced a new theory, the wave mechanics, or quantum mechanics, which was greatly superior to the older quantum theory. It forms in its large outlines a beautiful, consistent piece of mathematical construction. It is perfectly clear how to apply it to a great variety of problems, and the more accurately the calculations are made, the more closely the results appear to agree with experiment. In problems

dealing with the stationary states and spectra of atoms and simple molecules the theory has already been checked accurately enough so that no one seriously questions its correctness, and even in more complicated problems like the structure of solids it seems to be essentially correct. Not only is it a good theory in the sense of giving the right answers, but it is a theory of great mathematical interest and elegance. I find it incomprehensible that any one who is devoted to classical physics, and who takes the trouble to understand wave mechanics, should not thoroughly enjoy it. For its mathematics is very similar to that particularly satisfying branch of classical mechanics, the theory of sound, only it is more thoroughly developed and on the whole more beautiful. Any one interested in the aesthetic qualities of a theory should, I believe, find wave mechanics to be the finest branch of mathematical physics.

Since the development of quantum mechanics, then, the quantum theory has been in a rather satisfactory state. But electrodynamics is in a bad way. In a somewhat critical symposium like the present occasion, it seems appropriate to look at weak points in our present theoretical position, as well as strong ones. And the weak points in a theory, as I have suggested, are the places in which it refuses to give correct answers, or any answers at all, to questions regarding experiments. The situation of electromagnetic theory now, it seems to me, is almost as bad as that of quantum theory before the development of wave mechanics. It is a patchwork, able to give many useful results if treated properly, but far from logical or complete. It needs new, unifying ideas. Its fundamentals must be examined, and I believe they will be found to be weak. Many of the weaknesses become particularly obvious when we think about electromagnetic fields in ponderable matter, and that is the reason I have chosen that subject today.

In classical mechanics and electromagnetic theory it is easy to define an electric field. We place a small charge at a point of space, and measure the force on it. If we want to know how to measure the force, we can imagine an experiment using a spring balance; or we can have the charge placed on a small mass which will be accelerated, and we can measure

the acceleration, deducing the force from Newton's second law. Then we pass to the limit as the charge gets smaller and smaller. We find for small charges that the force goes proportionally to the charge, and we define the limiting ratio of force to charge as the field.

With the quantum theory, these experiments are clearly impossible. In the first place, we have nothing like spring balances on an atomic scale. If we try to measure a force by an acceleration, we fail for two reasons: we cannot measure the acceleration very accurately, and Newton's law does not hold anyway, on an atomic scale. Thus we cannot measure the force in any conventional way. And even if we could, we cannot go to the limit as charges get smaller and smaller, for there is no reason to suppose that charges smaller than the electron exist. The first difficulty is perhaps not very serious, for we know how to take account of forces, or potentials, in wave mechanics. There is a certain place in the theory where one puts in something called the potential; and finally out comes the wave function, depending among other things on that potential. We have a certain justification in saying that the correct force, or potential, is that which, inserted in the wave equation, gives results in agreement with experiment. That, I think, is the only way we can carry over the idea of forces, and electrical fields, into quantum theory.

The other difficulty, however, regarding the finite size of the electron, is not so easy to avoid. We can see that it is of really practical importance, by considering a simple experiment. An electron is emitted thermionically or photoelectrically from a metal surface. What is the force opposing it as it leaves? There are certain attractions of the surface atoms which must first be overcome, and after that we might think that no further force would act. An uncharged surface, according to ordinary electrical theory, exerts no electric field. But the electron polarizes the surface, or produces what is called an electric image. By that we mean that the electron, having a negative electric charge, pushes other electrons away from the surface, leaving a surplus of positive charge, which does have an external electric field. This field is of a simple sort: it is what would be exerted by an imaginary positive charge, equal in magnitude to the electron, and as

far below the surface as the electron is above it, the so-called electric image of the electron. The force exerted by this image on the electron is proportional to the product of the charges, or to the square of the electron's charge. Thus if we divide the force by the charge, we have something proportional to the charge, and if we could go to the limit of making the electron's charge smaller and smaller, this contribution to the field would vanish. That is the reason why we ordinarily say that an uncharged surface exerts no field at outside points. But the electron does not have a vanishingly small charge, and the force which remains, far from being negligible, is quite large, and is responsible for most of the thermionic work function. Are we going to call this force part of the electric field, or are we not? That is a question on which the present form of quantum electrodynamics is vague; such questions have not been thought about enough.

Our little problem of thermionic emission can be made to appear even worse. Suppose the electron moves quite fast, as it emerges from the metal, as it actually is almost sure to do. Then the other electrons of the metal, which are supposed to move out of the way to form the positive image charge, may not have time to carry out their manoeuvers successfully. From experiments on anomalous dispersion, in which light of various frequencies acts on bodies containing electrons, we know that the electrons react quite differently to different frequencies, and if the field changes too rapidly, they cannot follow it. It may very well be, then, that a different image will meet a fast electron from that encountered by a slow one, so that the force on it will be different. The electric field, in other words, may depend on the speed, or even on the whole motion, of the particle on which it acts. This is very different from the conventional idea, but it is practically forced on us by the finite nature of our test charges. Such questions become of imperative importance as soon as we think about ponderable bodies, solids and such things. For these bodies are full of atoms which can be polarized, and any motion of an electron will change the polarizations of all its neighbors, giving a force reacting on itself proportional to the square of its charge, a force which would vanish in the limit of zero charge, but which is of extreme importance with electrons.

It is these forces, of course, which are concerned in questions relating to dielectric constants, indices of refraction, and such things, and corresponding magnetic effects come in with problems of magnetism. No conscientious attempt to think about such a problem can avoid the type of difficulty I have just spoken about. But it is typical of the unsatisfactory thinking which has led to the present theories of quantum electrodynamics that in spite of the great amount of work which has been done, and the many papers and books written on the subject, one will hardly find a mention of the existence of the dielectric constant among them.

Electromagnetic fields have usually appeared in conventional quantum mechanics in two quite different ways. First there is the Coulomb interaction between pairs of particles, which is met in the structure of atoms and molecules and solids. This is handled in Schrödinger's equation as an operator. In the Hamiltonian operator, H, met in the equation $H\psi = E\psi$, we include a sum of terms for each pair of particles, each equal to $e_i e_j / r_{ij}$, the product of the charges divided by the distance between, the conventional electrostatic term. Secondly, there is the interaction between particles and external radiation fields. If such a field is represented by scalar and vector potentials ϕ and A, usually considered to be functions of position and time, we introduce a term into the Hamiltonian given by the product of the various charges times the potentials ϕ at their positions, with analogous terms for the vector potential. Merely to regard the potentials as external perturbations is not enough, however. It accounts for the effect of the field on the charges, but not of the charges on the field. To remedy this, the radiation field is really introduced as a separate part of the system, able to have stationary states and quantum numbers of its own, and therefore able to receive energy from radiating atoms, as well as to give up energy to them.

It is only natural that the quantum electrodynamics has made some effort to unify these two quite different treatments of electromagnetic fields. It has chosen to proceed from the radiation fields, preferring to regard them as more fundamental. The radiation is analyzed into plane waves, and then as a result of considerable legerdemain it is shown that

part of the action of these plane waves results in the Coulomb interaction between particles, the rest in the action of radiation. The resulting theory is able to explain a number of experiments, but it meets much trouble. In the first place, there is the difficulty of the "self-energy" of the electron. In finding the Coulomb interaction, the sum of terms $e_i e_j / r_{ij}$, or the products of charges divided by the distance between the ith and jth charges, stubbornly refuses to be limited to the cases for which i is different from j. And yet the terms for which $i = j$ obviously are infinite, since the distance r vanishes in this case. The trouble occurs in classical theory, and is handled by assuming a finite radius for the electron, inserting essentially this value in place of the offending denominator. But it has been hard to adapt this expedient to the quantum theory. The trouble is not met in the ordinary formulation of Schrödinger's equation, for there it is simply postulated that the Coulomb interactions exist only between different particles.

Then there is another sort of lack of convergence in the theory. In many cases, when the analysis into plane waves is carried out, the series diverge in the limit of short wave lengths, or high frequencies. This is sometimes considered to have a physical meaning; but I suspect that it is merely a result of the mathematically inept attempt to describe radiation fields by plane waves. All kinds of difficulties of convergence are met as soon as one tries to expand a spherical wave in terms of plane waves. A point source of light is a singularity of a difficult sort for this type of expansion. Furthermore, even if the function can be expanded directly, it is customary to differentiate the result term by term, and as with a Fourier series this is likely to destroy whatever convergence may have been originally present. The attempts of quantum electrodynamics, on the whole, have not been distinguished by success.

Yet we must surely seek some unification between the electrostatic interaction of neighboring particles and the action of light waves. A glance at the problem of electromagnetic fields in solids shows that there is even less distinction between them than ordinary quantum electrodynamics supposes. Suppose an external field acts on a solid. Then

it is not this field alone which acts on any atom or electron of the solid. Each atom is polarized by the field, and each in turn reacts back on every other atom, in addition to the direct effect of the external field. It is this reaction of polarized atoms which is so well known in dielectric and magnetic phenomena, and which we described earlier in talking about thermionic emission and image forces. And yet the polarized atoms acting on one of their number form all intermediate stages between the near neighbors, whose fields are practically electrostatic and can surely be handled by Coulomb methods, and distant atoms, whose fields are pure radiation, and surely should be handled by radiation methods. No theory can be successful which makes any distinction between the two types of field. Quantum electrodynamics has conveniently closed its eyes to problems of this type, acting as if there were nothing in the universe but single atoms in empty space. In doing this it has closed its eyes to the one type of problem which seems to form a bridge between two quite different methods of handling analogous questions. The history of science shows that progress is most likely to be made in just such unifications of apparently contradictory methods. Failure to attempt to do this, I suspect, is the most important reason for the comparative failure of quantum electrodynamics.

A good deal of the difficulty seems to me to come from a fundamental failure to distinguish between two different things which we may mean by the electromagnetic potentials or fields. We can see the distinction clearly from the simple examples we have used. First, what do we mean by the scalar potential in the electrostatic case? We mean an operator entering into Schrödinger's equation. The potential at the ith electron is $\sum_{j \neq i} e_j / r_{ij}$, a quantity depending not only on the coördinates of the ith electron, but on the coördinates of all other electrons, regarded as operators. On the other hand, what is the scalar potential in the radiation case? It is a function $\phi(x, y, z, t)$, depending only on the coordinates of the point where it is to be computed, and the time. Now we must not forget that even a radiation field comes in the last analysis from atoms and electrons, and by analogy we should expect the potential here, as before, to be an operator depending on the positions of the other electrons, perhaps at retarded

times, and not just a function explicitly of position and time. How do we mentally justify the neglect of these electronic positions? There is only one possible way. We must say to ourselves, the potential ϕ of the radiation field is in some sense the average or sum of potentials sent out by many electrons, in which the individual peculiarities of the various particles are ironed out by averaging over a great many individual processes. It is a field very much like that which we are familiar with, in atomic structure, as the self-consistent field of Hartree and Fock. It seems to me likely that the radiation problem is really to be handled in some such average, approximate way as that, that it is such a field that satisfies Maxwell's equations, and that it is an entire reversal of the logical order to try to derive from it the detailed interactions between electrons, in the form of operators, such as is required in Schrödinger's equation. Let us examine in succession what we may call the operator field, and the average field, and see if our ideas are not clarified if we regard them as entirely different things, not just as the two limiting cases of a single field, in the limits of electrostatics and radiation.

First consider the operator field. By this we mean the way in which the electromagnetic interactions of particles enter into Schrödinger's equation, as operators. The first approximation, as we have said, consists of electrostatic Coulomb terms between pairs of particles. But this surely is nothing more than a first approximation. It is not even sufficient to give the first order magnetic interaction between moving electrons, so that it could not explain as simple a thing as the self-induction of a circuit. We can easily get what seems to be the next term of the approximation. It is a sum over all pairs i, j, of terms like $e_i e_j (p_i \cdot p_j)/c^2 m_i m_j r_{ij}$, where p_i, p_j are the momenta of the two particles. This comes from the interaction of the momentum of one electron with the vector potential of another, and leads to the ordinary magnetic forces between moving charges, or between currents. But it is obvious that when this appears as an operator in Schrödinger's equation, the p's will have to be replaced by differential operators, so that it is clearer than ever that the electromagnetic actions, in this part of the theory, are in the form of operators, not of functions of position and time.

Beyond this magnetic term it is not obvious how to proceed, and not certain that we can proceed at all. Certainly retardation must be worked in, and it is not clear whether a Hamiltonian, essentially in terms of action at a distance, can be set up in a case of retardation. If it could be set up, there would probably be quantities in it which would take the place of electromagnetic potentials, and in some highly symbolic way would satisfy Maxwell's equations; but I do not think they would be very much like the ordinary quantities of electromagnetic theory. It is presumably in such an exact theory that we should look for the answer to the question of the mass of the electron. Of course, Schrödinger's equation meets no difficulty of infinite self-energy. In place of the term e_i^2/r_{ii} in the potential energy, which would be infinite if interpreted literally, the relativistic form of the equation would have the term $m_o c^2$, the self-energy of the particle of mass m_o. Similarly, in the magnetic term which we have just written, in place of $e_i^2 p_i^2/c^2 m_i^2 r_{ii}$, we have the ordinary kinetic energy term, $p_i^2/2m_i$. That is, in the first case, we replace r_{ii} by $e^2/m_o c^2$, and in the second by $2e^2/m_o c^2$. No doubt, if Schrödinger's equation can be generalized in some such way as has been suggested, these terms, representing the self-energy of the electron, will still be handled in this way, but no doubt more elegant ways will be found for describing the steps.

The extended electromagnetic operator, and extended Schrödinger's equation, which I have just described, have not been set up, so that we cannot construct a complete theory. It seems much more promising to try to set up an average, or self-consistent, theory, sufficient to answer our ordinary questions about electromagnetism, in particular about the electromagnetics of ordinary matter. Let us look a little at ordinary electromagnetism, and see why it has the aspect of an average, or self-consistent, theory.

The method of the self-consistent field starts by replacing the operator field acting on an electron, which depends on the positions of all other electrons, by an averaged field depending only on position. This averaged field is generally different for each electron. Then the motion of the electron is found in this field, and its resulting wave function and charge

density computed. From the resulting charge densities of all electrons, in turn, we compute the averaged field, by ordinary electrostatics, and demand that the result be self-consistent, or that the final field equal the initial, averaged field. Now the very first step in this process is just what we have seen is done in ordinary radiation theory: we replace the operator field by an averaged field. But the resemblance goes further. Suppose we have solved the self-consistent field problem for a piece of matter in the absence of radiation, and that then we expose the whole piece of matter to external radiation. By perturbation theory, or some modification of our calculation, we can find the motion of each electron in the presence both of the original self-consistent field and of the external field. But the resulting polarization of each atom will in turn change the field acting on each particle. Really the change in field acting on a particle, brought about by the external field, will have two parts to it: first, the external field itself, and secondly, the field resulting from polarization of other particles. And the condition for the self-consistency of the problem is now that the resulting motion of each particle, brought about by this combined field, shall just yield the polarization which in turn influences the field. This is exactly analogous to the ordinary condition in the Lorentz-Drude electron theory, the condition determining the relation between P and E, which leads to the refractive index. No one who looks through that familiar derivation, with the present point of view in mind, can fail to see that the steps are exactly those of a self-consistent field problem.

It is plausible from what has been said that problems in dispersion, dielectrics, magnetic properties, and such things can be handled in terms of a self-consistent or average theory. Properly, such a theory should come as a form of average from an exact theory, as the theories of Hartree and Fock come from Schrödinger's equation. It may be simpler in practice, however, to start directly with the average theory, and to try to deduce its form. Jordan and Klein a number of years ago made a promising beginning in this direction, setting up a theory which operated in three-dimensional space rather than configuration space, and was formulated

in close analogy with classical electromagnetism. They worked with integrals, or averages, reducing to something similar to Hartree's theory in a first approximation, but containing the elements necessary to yield a final exact result in agreement with Schrödinger's equation. This theory should be capable of handling problems in static or slowly oscillating fields in matter without great extension. It is more difficult to see how to extend it to problems involving radiation, but it seems to me that an attempt to do so, guided where necessary by correspondence principle and classical analogies, would be well worth making in the present state of quantum theory.

In conclusion, then, I should like to repeat the statement, which most physicists will agree with, that electromagnetic theory is in a much worse state than quantum mechanics. And I should like to suggest that one useful approach to the problem may be through the study of electromagnetic fields within ponderable matter. A great deal of attention of course has been paid to the more abstruse electromagnetic phenomena, pair formation, and such questions. We have tended to lose sight of more elementary and familiar problems. But it may well be that these elementary questions may play their part, along with the more involved ones, in leading to a final understanding of these perplexing difficulties.

Airfoam Seat Cushions.—In the industrial survey section of *Invention*, Vol. III, No. 1, there is briefly described a product of the Goodyear Tire and Rubber Company known as Goodyear Airfoam, which should especially appeal to comfort seekers. It is a cushioning product to be used in seats in the automotive field. Stressing rider comfort, greater safety due to decreased bodily fatigue, and greater economy due to the virtual elimination of repair and replacement charges, the company is entering the field on a large scale. Airfoam is dustless and does not exude lint in any form. The porus nature of Airfoam cushions and backs, it is explained, insures comfortable temperature of the seats due to the fact that every bodily movement forces air to circulate throughout the entire structure of the seat. Under pressure the air in the honeycombed rubber escapes and the seat adjusts itself to any body position, resuming its molded shape immediately when the pressure is removed. Vibration is absorbed, effecting a notable reduction in driving strain during long periods at the wheel. The material is 95 per cent. pure latex and is the result of extensive chemical and engineering research. The liquid rubber is beaten into a froth containing millions of interconnected air cells, then cured in molds designed to fit the needs of seat manufacturers. It is also available in mattress form suitable for sleeper cab accommodations. Unlike ordinary sponge rubber, which is solid sheet rubber treated with chemicals to produce expansion and swelling of uneven porosity, Airfoam is treated in such a manner that it has no odor and is slightly antiseptic, repelling moths and vermin.

R. H. O.

THE SIGNIFICANCE OF ATOMIC ENTITIES IN MODERN ATOMIC STRUCTURE.

BY

W. F. G. SWANN, D.Sc.,

Director, Bartol Research Foundation of The Franklin Institute, Swarthmore, Pennsylvania.

INTRODUCTION.

It may horrify the believer in stern reality in physics to assert—as has been asserted—that the laws of physics are made by man; and, yet, it is a curious fact that those who cry most loudly for realism and for limitation of discussion to "hard facts" are the least content if one confines his attention to those realms alone, and are the most guilty offenders in building in thought a fabric which they may worship with a consciousness that it is real, even though none of the concepts which they invoke would, in the last analysis, satisfy any criterion of reality which they themselves would formulate. They delight to speak of what a thing would look like if we could see it; and, they are ready to brush aside any argument that "seeing" might seem to have no meaning in the ordinary sense of things as applied to such a concept.

REALITY AND VIEWPOINT.

It is strange how the same mind will regard as artificial in one field a method of thinking which it regards as natural in another, and vice versa. Thus, in classical physics, the velocity of a fluid is, in many cases, derivable from a potential φ obeying Poisson's equation, which, in this case, is the representative of the equation of continuity. The picture which the mind likes as controlling the phenomena is that of the fluid wending its way along in such a way that each element of it is subjected to the forces from its neighbors, which push it here and there in such a manner as to maintain the aforesaid equation of continuity and, through the appropriate constraints determined by the boundary conditions,

realize a unique solution for the motion. The fact that the ultimate velocity at a point is expressible as a vector obeying the law of inverse squares in relation to certain sources and sinks which may be present is apt to appear as artificial, and of purely mathematical significance. The artificiality is enhanced still further if there should be no real sources and sinks, and the mathematician should draw a perfectly arbitrary boundary in the fluid and proceed to express the velocity within that boundary in terms of the law of inverse squares (or closely related law [1]) as applied to certain appropriately defined sources and sinks on the arbitrary boundary, by the method so familiar to mathematicians in the use of Green's equivalent stratum.

However, the electrical potential in an electrostatic problem is expressible by the same Poisson's equation, with analogous conditions for determining uniqueness, and the solutions are of the same form. Here, however, the picture of law of inverse square contributions from electrical charges is very agreeable to the mind. Even Green's equivalent stratum is more palatable; and, any attempt to picture an adjustment of things in the medium according to such principles as seemed natural when Poisson's equation was an equation of continuity seem now vague and artificial. Here are two problems, A and B, a hydrodynamical problem and an electrical problem. That which is artificial for A seems understandable for B, and vice versa. Yet, both problems are controlled by the same fundamental law.

In any new theory or extension of an old one the mind seeks to preserve and impose upon the new theory the concepts which it has been accustomed to work with in older fields where they seemed the natural tools, and where they became cemented in the mind. These concepts pursue the theory to which they belong to the grave and seek a place in its successors where they wander about as ghosts, giving here and there a suggestion from their past lives, but causing frequent trouble and existing themselves in a state of what we may call mathematical discontent because the clothes which they wore when they were alive, and which then were in harmony with

[1] The parenthesis refers to the hypothetical doublet distribution in Green's equivalent stratum.

their surroundings, no longer have place or meaning in the new realm, where it is hard enough to tolerate them as spirits.

PARTICLES ULTIMATELY UNNECESSARY.

If one should maintain that in the spirit of the modern quantum theory one could, in principle, correlate all the phenomena of nature covered by that theory without thought or mention of the word "particle," he would probably be dubbed by many as a maniac or a metaphysician, which latter term seems to be regarded by many as the scientific description of the former state. Yet, a close analysis of the situation will show that, in the actual mechanism of the quantum mechanics, while the term "particle" is frequently used, it is only used as a starting point to inform one's readers of the problem to be attacked, by reminding them of what the problem was in classical physics. In the subsequent development, not only are the properties of the particle eliminated, but even the "particleness" of the particle disappears. It returns occasionally to the picture, but without any properties or functions other than that associated with the expression of the probability that it was somewhere, or that it had a certain amount of momentum. Even if, in line with the spirit of the quantum theory, an experiment has been performed according to which, in the usual phraseology of words, the particle has been caused to assume a certain definite state of momentum and we proceed to ask the question "What of it?", and command the particle to do something on account of its momentum, it can do no more than say "Excuse me, while I arrange that this be done," after which it modestly disappears from the picture until a mathematical problem leading to the next stage of solution on the journey to the point where an observer writes down some number in his notebook has been solved; and, in this next stage of solution the particle returns once more out of oblivion to take the bow for what has been accomplished.

Even in those experiments which are most characteristic as evidence of the existence of real atomic entities in the form of particles, the story of that evidence becomes told by the quantum theory in a manner which does not invoke the "particleness" of the particle. Perhaps the most simple and

concrete of these experiments is that which we describe in terms of the passage of an α particle through a Wilson cloud chamber. Here, it will be argued, we have almost seen the particle. There is a streak of water drops in a well-defined line. There are none or hardly any to the right and there are none or hardly any to the left. Something most assuredly went along the path where that track is, says commonsense; and, yet, the quantum theory tells the story of this phenomenon without the invocation of any such particle. The story is that of a plane wave of infinite extent traveling in a certain direction through a lot of atoms. In line with the spirit of my present remark, I could even avoid saying "through a lot of atoms," and could say that the problem is one of a ψ distribution which can be analysed into a plane wave with a number of states of beknottedness in it.[2] However, having reminded you that I can say this if I wish, I will, in actuality, speak of the ψ wave as passing through these atoms, and if you accuse me of inconsistency, I shall say that it is for your benefit that I committed this sin. In using the term "you," I do not, of course, refer to the distinguished audience before me, but to that hypothetical group of beings whom my mind pictures as occupying the seats, beings who are all very square-jawed and materialistic and in a frame of mind to dispute everything that I say.

Well, the ψ wave results in ionization of one of these atoms, and then a remarkable thing happens. The mathematics causes that atom to become a center of disturbance, for an additional contribution to the ψ function, which contribution travels out in a very small-angled cone whose axis is in the direction of the normal to the ψ wave which produced it. The total ψ function in this cone is such as to increase greatly the probability of ionization of another atom which falls within it over that for any atom which falls outside it. It is as though the first atom sent out in a cone, in the direction of the original wave, a message telling of the terrible thing that had happened to it, which message intimidated

[2] Ideally, of course, the wave is one in a space of very many dimensions on account of the presence of all of the atoms; but, our simplification of language will do no harm for the purpose here in hand.

the next atom in that cone.[3] The ionization of another atom in the cone sends out another cone, so that the phenomenon is propagated in a straight line. In the case of an infinite plane ψ wave, it was an accident that the particular atom which we started with should be the one ionized. Truly, every other atom which is ionized is ionized by accident; but, it is much less of an accident that those which are in line with the first atom ionized shall be ionized than it is that some other atom out of that "beam" shall be ionized.

The essence of the above remarks is to the effect that the story up to and including the ionizations of the particles, including the rectilinear character of the track, can be told without invocation of anything which looks like our old concept of a particle—an α particle—traveling through the gas. It is an ideal to believe that in analogous, more complex, manner the quantum theory can follow the story of what we call the condensation of water on the droplets, the formation of the images of those droplets through our microscopes upon the retinas of our eyes, and if we are bold enough from there through our nerves to our brains and to the ultimate story of our writing down in a notebook a qualitative or quantitative description of what we say we have seen. The same ideal, when extrapolated over all the phenomena of physics, can, in principle, at any rate, embrace the story of all those relations which are concerned with what we call the laws of physics and the verification of those laws.

THE PRACTICAL VALUE OF PARTIAL CONCEPTS, ETC.

Before pursuing still further the path of abstract idealism, however, and for the purpose of providing a "breathing spell" in what may seem the realm of unreality, we may pause to enquire whether there may not be some *practical* necessity for the perpetuation of such concepts as electric charges with their old properties. I think there is such a practical necessity, but I also think that we must admit that its utilization is a strict breach of logic, unless, indeed, the principle invoked in this breach be itself admitted as part of the realm of what we call theory, with the necessary curtailment of meaning to

[3] Or perhaps it would be better to regard the cone as a reinforcement in persuasion for the second atom to become ionized.

the story to which the more ideal formulation would presumably lead if that story could be told.

To fix our ideas, suppose that we are concerned with a problem in which, in ordinary language, a beam of electrons strikes an X-ray target, emitting X-rays which generate a photoelectric effect somewhere else, with the result that some electrons go into a Faraday cylinder connected to an electrometer with lamp and scale. Presumably the story of everything that happens could, as an ideal, be represented in abstract language, without the invocation of particles, from the point at which one puts down the switch to start what we have called the beam of electrons (but, perhaps, should not) to the reading of the galvanometer scale. Even beyond these points we can imagine the stories prevailing in this language. However, the problem really divides itself into two parts, the part concerned with the X-rays, the photoelectric effect, and so forth, where the quantum theory does its really special stunts and where the classical theory fails, and another part concerned with the story following the entry of what classical physics would call the electron into the Faraday cylinder. The latter part of the story, provided that we start with the concept of an electric charge in the Faraday cylinder, was very simple on the classical theory; and, while the quantum theory raises doubts as to the details of the process, we have no doubts about the conclusions, nor will the quantum theory protest audibly against those conclusions. We apply the laws of electrostatics in relation to the so-called capacities of the Faraday cylinder the electrometer, and so forth, and we proceed to our results. Think, however, what a terrible time the quantum theory would have to handle this matter in the complete sense. First of all, there would enter those conditions which, in classical theory, result in the appropriate distribution of electricity between Faraday cylinder and electrometer. Herein lies a terrible story, because we know that if there were no electrical resistance, electrical oscillations would take place between the Faraday cylinder and electrometer for ever, after the electron had entered. A statical state would only by realized on account of resistance. However, the quantum theory in its idealism could not invoke, but would have itself to discover in the theory of its own

processes as applied to the conductors concerned, the existence of this resistance. Finally, it would have to discover and bring into realization in its story all of those matters concerned with the elasticity of the fiber which supports the needle in the electrometer. It would have to discover Hooke's law, to say nothing of the elastic principles involved in all that is concerned in the instrument holding itself together. In other words, it is difficult for the quantum theory to keep itself logically straight without telling not only the story of what is needed, but the story of everything else. Truly, some of the complexities can occasionally be short-circuited, but the net result is that the classical theory involves a very much more convenient process for handling the story of the electrometer, etc., than does the quantum theory, so much so, in fact, that if there had been no problems in nature other than those of electrometers, and the like, there never would have been a quantum theory.

In making the transition from quantum theory to classical theory as a matter of convenience, however, we must realize that there is a discontinuity in logic—a complete ending of one story before it is completed, and a cementation to it of another story made out of potentially discarded material salvaged from the attic where are housed the paraphernalia of old theories.

THE "PARTICLENESS" OF THE PARTICLES.

I am aware of the fact that it is customary to state a principle of complete complementality between particles and waves in the sense that nature can be described equally well in terms of either. Without, for the moment, questioning the validity of this statement in all of its ramifications, I wish merely to emphasize that, in such a formulation, the properties attributed to the particles are entirely different from those associated with them in the older theories. The particles have no "forces" between them—they simply obey certain laws of what one may term impact. The particles are not assigned any size. They are talked of as having position and momentum; but, even the position is a vague kind of thing. The phenomenon of looking at such a particle even with a gamma-ray microscope, without invoking for it properties other than given by the quantum theory, is a process having

but little physical significance. One thinks of a kind of black dot on which the microscope is focussed; but, in the story of seeing it we must not invoke any of the mentally satisfactory pictures of light waves forming an image of it in the microscope. The story of seeing it must be told in terms of the quantum laws of collision of material particles and photon particles; and, quite apart from all questions involved in the "uncertainty principle," the phenomenon concerned in what is meant by "seeing" it, is so far removed from anything which the practical mind likes to contemplate that one may well say that the "particleness" of the particle has completely evaporated. It might as well have been an entity extended throughout the universe.

PARTICLES IN ATOMIC NUCLEI.[4]

To all of the foregoing one may protest: How is one to talk about the structure of atoms unless one speaks about electrons, protons, and the like? What position do these entities occupy in a language which minimizes talk of all particles? There is something to be said for trying to clear up some of these matters. When we are presented with an array of entities such as those referred to, and raise with ourselves the question as to whether or not certain of them exist in the nucleus of some atom, we are apt to encounter ambiguities of meaning founded upon whether some complicated particle such as an alpha particle exists in the nucleus as an individual entity, or whether it is to be thought of as made up out of such things as protons and electrons. We are confronted, for example, with the question of whether the proton is to be regarded as a neutron plus a positron, or as an entity distinct in itself. In some atomic transmutations we may raise the question as to whether some of the particles which we observe were in the nucleus originally or whether they were formed in the process of its disruption. These uncertainties of meaning become enhanced when we try to picture a structure for the nucleus. In classical theory we became confronted with the problem of putting into a nucleus of certain limited size a number of entities whose classical

[4] This Section, and the next one entitled "Significance of the Existence of Different Kinds of Atoms," have been taken from my article "Nuclear Phenomena and Cosmic Rays," *Ohio Journal of Science*, 35, 311–342, 1935.

dimensions are so great that they would have to overlap in order to accommodate themselves. In wave mechanical theory the various entities appear dissolved in a kind of mathematical fluid, from which any one may be precipitated at any desired moment for inspection by a suitable reagent which takes the form of a mathematical operation. Under such conditions it may not be out of place to pause a moment, from time to time, for the purpose of deciding with ourselves what it is that we are talking about, and in particular as to what meaning is to be attached to the statement that a certain entity, let us say a proton, is, or is not, inside a nucleus; and, if in the nucleus, as to whether it exists there as a fundamental entity or as something which is to be thought of as made up of two entities—for example, a neutron and a positron. To my mind, definiteness of statement can only be secured along such lines as the following: When we wish to introduce into our problem such an entity as an atom, that atom has a certain mathematical representative which is responsible for all of its activities in the theory. That representative may be a Hamiltonian function. This function figures in some differential equations which are representative of the theory of the subject. In these differential equations are certain coördinates which are potentially representative of all of the entities which may figure in the discussion of all that may occur.[5] Any particular solution of these equations represents a *state* of the system. Some of the coördinates representative of certain entities may be absent *in any particular solution*. If such is the case, the entities concerned are absent in the state representative of that solution. It may be possible in the case of some particular solution, representative of a state, to make a transformation of coördinates in such a manner as to reduce the number of the coördinates. Then, entities characterized by the new coördinates may be said to exist in the state concerned.[6] Thus,

[5] The entities are supposed chosen so that they represent the minimum number necessary for telling the story of all that may occur through the agency fo the accompanying mathematical theory.

[6] In this manner, for example, the coördinates of a suitable number of protons and electrons may, in certain cases, be mathematically welded into the much smaller number of coördinates of an alpha particle.

suppose among the fundamental entities we had neutrons and positrons, but not protons. Then a free proton, or a proton in an electric field, for example, is to be considered as a system which, primarily represented by a function of the coördinates of a neutron and a positron, has been found, through a transformation of coördinates, to be represented by new coördinates to the extent of half the number. If, now, we raise the question of whether in an atom there are protons, or neutrons and positrons, the question at issue is whether the same transformation which reduced the number of coördinates in the single case of neutron and positron, above cited, will now function in similar manner to reduce the number of coördinates in the function representative of the state of the atom in question. If it does, we may say, by hypothesis, that a proton exists in the atom. It matters not whether some experiment reveals a proton as apparently ejected from the nucleus. The story of whether the said proton was, or was not, in the nucleus has no meaning except in the light of some definition; and the definition I have suggested seems to be the only obvious one available.

SIGNIFICANCE OF THE EXISTENCE OF DIFFERENT ATOMS.

While speaking of these rather general philosophical matters, one cannot resist the temptation to speculate a little further upon the significance of the quantum theory. At present we have a separate differential equation for each atom—the atom in question being characterized by the form of a certain function (the Hamiltonian function) entering into the equation. The different states of the atoms are characterized by the different solutions which are to be permitted as acceptable according to a certain criterion—the criterion of finiteness, single-valuedness, etc. May it not be that the different atoms themselves are to be regarded as representative of "quantum states" founded upon a yet wider idea of quantization? Many possibilities suggest themselves. Speaking generally, atoms of successively increasing atomic number are characterized, in part, mathematically, as systems of increasing numbers of coördinates. May it not be that there is one fundamental differential equation, or set thereof, involving, perhaps, in the initial instance an infinite number

of coördinates, but of which only certain of the solutions satisfy the criteria demanded, finiteness, single-valuedness, etc., for example? It may be that all the solutions involving certain specified numbers of coördinates would be unacceptable under the criterion stated. Presumably those involving more coördinates than are found in the heaviest known element would come into this category.[7] The individual solutions in which the number of coördinates were limited would naturally satisfy, in addition to the general differential equation involving the multitude of coördinates, certain special differential equations involving only the coördinates occurring in the solutions. Presumably these special differential equations would be those which we now think of as characteristic of the individual atoms.

THE PRACTICAL POINT OF CORRELATION WITH EXPERIENCE.

In discussions such as the present, it is frequently easy to tear down; and, one must be on his guard against raising philosophical difficulties which may not really be relevant to the story in hand, in spite of the actuality of their existence. Thus, I can argue that I have no criterion that I have an audience this afternoon. All I am concerned with is my own existence and my own impressions, etc., etc., I cannot even prove that this state of mind is irrelevant for everything that might be said and yet I feel that it is irrelevant for my purposes this afternoon. I at least, indeed, have a strong hope that you are here and I am under the impression that I am talking on the basis of that assumption.

I raise, therefore, the question as to what we are to regard as the points in the mathematical structure which tie up with observation. It seems to me that we may reasonably take quantum "states" as things which may be regarded as being observed. In other words, realizing that the use of a theory involves the setting up of a one to one correspondence between elements in a mathematical structure and concepts in a physical structure, it appears that the elements in the mathematical theory relevant to the realization of a "state"

[7] Except in so far as solutions involving many coordinates may exist, but in forms tending as limiting cases to conditions representative of a number of independent atoms, and so to solutions depending upon separate sub-Hamiltonian functions.

are satisfactory for making this concept the point of correlation with the observation of an experiment being completed, for example. Thus, there is harmony between mathematical formalism and intuitive desires in realization that the practical end of a spectroscopic experiment is the formation of the latent image of a line upon a photographic plate. This line is chemically symbolized as a change of state of a certain silver compound. We are content to leave the problem there and not worry concerning the quantum theory of the act of measuring the position of the line, and meditating upon it. Indeed, it is not absolutely necessary to carry the story up to the point of the quantum theory of the chemical reduction of the silver salt, so long as the theory is made to end at a place where the logical path to the reduction of the silver salt is a path freed from perplexity, however much it may be encumbered with complexity.

In a sense, the spirit of the quantum theory is to the effect that the whole universe is a succession of states. For my own part, I do not like contemplation of the idea of "doing an experiment." I hate to invite all the mathematical accusations and responsibilities resulting from the rôle of experimenter. I fear that if I wish to talk rather precisely about a problem at a certain stage and am in danger of saying something unpalatable to the mathematician, he will arise and side-track all his difficulties by accusing me of having spoilt everything which I wanted to talk about by doing the experiment which gave me the data to talk about it.

AVOIDANCE OF EXPERIMENTS.

And so, in this state of mind, I prefer to go on a sit-down strike. I shall form a union of all experimenters. We shall set up certain motors and other appliances to keep the laboratory going by itself, automatically, and shall simply sit down and watch. The laboratory may not of itself do all of the experiments which, in the run of eternity, would be done by our aid; but we can arrange that its output is sufficiently rich to provide an interesting story for the quantum theory to talk about. It will readily be realized that this robot laboratory will even make discoveries. In our sit-down strike, we, the experimenters, shall simply observe with our

eyes and think. It may be that the mathematician will be suspicious of us even in this capacity, and may accuse us in that our very thoughts disturb the progress of affairs. For this reason we shall fit up certain automatic devices for developing the plates recording the positions of lines on scales, and so forth; and, that we may relieve ourselves of all responsibility, we shall arrange automatic typewriters which are also part of the scheme and which will type down on paper certain numbers. For a moment I take you into my confidence by telling you that these numbers are concerned with such measurements as the positions of things which have experienced changes in state and which really constitute ultimately the things of interest in my meditations. I realize that the introduction of all of these typewriters, with their quantum theories of operation, is a terrible complication for the mathematician; but, after all, if the quantum story is complete the complication should not be one of perplexity but only of complexity, and in this I shall not pity the mathematician, since it was his fault, through his accusations, that caused me to incorporate these appliances. Having incorporated all these things and left matters to themselves, the laboratory will proceed to evolve typewritten numbers with certain correspondences between them in the matter of simultaneity and so forth, since times have also been written down by the arrangements cited. Later, I shall have these numbers sent to me, and it will be my purpose so seek correlations between them. After I have received the numbers I may, for the purpose of giving myself hints as to how to proceed, inquire what instruments produced them. However, such inquiry is only for convenience, and is not fundamental. In the correlations which I make I hope to find the story of nature in so far as the laboratory has researched upon it. I shall take the correlations to a mathematician and it will be his business to paint them upon a frame of substructure which is the quantum theory.[8] From

[8] I may here deal with an element which evolved in the discussion and concerning the necessity of repeating experiments in order to provide the data necessary to give meaning to ψ as a measure of probability. In the spirit of the foregoing these repititions of the experiment are all part of the continuous progress of events carried on automatically by the robot laboratory.

this stage on, I shall cease to talk in such pedantic terms since I hope to have set the stage for return to this language in case anyone should question the meaning of some of my words now to be uttered.

And so once more I say that I observe quantum "states" and that the interest of physics lies in seeking correlations between these states. My observation of these quantum "states" is really the equivalent of the so-called macroscopic observations which physicists make, and which it was the object of classical theory to harmonize in the laws of determinism.

SIGNIFICANCE OF INDETERMINATION.

Now in the harmonization of a set of data various interesting matters present themselves. In classical astronomy, the orbits of the planets are uniquely defined in terms of the positions and momenta assigned at any time. If, however, the differential equation of planetary motion had been of the third order, the assignment of two initial conditions would have been insufficient. The motion would have been restricted to some extent; but, there would have been a wide "principle of indetermination." Concerning this no one would have been surprised. Indeed, when the mathematician speaks of the conditions that a set of differential equations shall have a unique solution, he refers to the fact that it is necessary to assign a certain number of quantities in addition to the differential equation, in order that the complete story shall be told. The higher the order of the differential equation, the less it says, and the more is said in the initial conditions. Indeed, the statement so current in the restricted theory of relativity to the effect that similar experiments performed on relatively moving systems give identical results has to me no meaning unless one says what he means by an experiment; and, in the sense of classical physics, an experiment is an assignment of those things which must be assigned in order that, through the differential equations of the subject, a unique result shall be predicted. The beginner in the relativity theory having encountered much difficulty in building up equations which are invariant in the restricted theory, is astounded when he hears the general theory talking

of relations which are invariant under *all* transformations of coördinates. The equations of the general theory acquire this apparently remarkable property by the characteristic of saying less than those of the restricted theory, leaving more of the story to be told in the boundary conditions. By saying sufficiently little it is possible to increase the generality of truth of that which is said. Indeed, it is obvious, for example, that if a differential equation should be of infinite order, it would say nothing, which is a sufficiently non-comprising statement to be invariant under all transformations of coördinates.

A cogent element in our discussion, therefore, concerns the question of whether it is possible to set up a deterministic scheme between macroscopic quantities alone. In other words, the question which is really of interest is this: Is it possible, by the specification of macroscopic, that is, really assignable magnitudes, alone, to build up a theory which predicts uniquely macroscopic results, or is it necessary to include, even for the macroscopic results, the assignment of some sub-macroscopic quantities? If, indeed, the latter situation is the case, then the scheme is, in essence, indeterministic. To illustrate by a simple case, I may say that we can predict the macroscopic temperature of a gas in terms of the macroscopic pressure and macroscopic volume for a macroscopically adiabatic system. However, we cannot, from macroscopic measurements predict the motion of each molecule and, what is important, we do not need to invoke the individual motions of the molecules to predict—of course, to the degree of classical exactitude—things about the pressure and temperature of the gas.

Another element in the question of macroscopic determinism concerns the question of whether the quantities which we seek to assign in an endeavor to secure determinism may be the wrong quantities in the light of the particular theoretical substratum which governs the story. Thus, for example, in classical astronomy, before we knew of the law of gravitation, it might have occurred to some of us to seek empirical relations which would describe the orbits of the planets in terms of their initial positions and temperatures. I have no doubt that if these things were less visible and if

our previous mental activities had been concerned almost exclusively with positions and temperatures these would have seemed the natural things in terms of which to develop the consequences of any theory.

While I admit there is not much to be gained by contemplation of the anomaly, I can never escape surprise from the existence of the anomaly presented by one who assigns initial conditions for beams of electrons by specifying potential differences between filaments and slits and then remarks upon the apparently strange consequences of indeterminism in terms of such specifications, when even the classical equations of motion of electrons were theoretically not of the second but of infinite order. It is still more surprising to contemplate specifications of initial conditions in the case of ψ functions in terms of quantities measured for electrons by voltmeters, where the picture of the initial conditions specified is so different from that which enters our mind in thinking of the waves.

Another rather strange situation arises in making so much of the effect of measurement in the case of such things as velocities of electrons. While I am willing to go to the limit in emphasizing the futility and even meaninglessness of the position or velocity of an electron in the sense understood in some of the cases cited, I would point out that hardly ever does one seek to measure such quantities by microscopes and the like. Even a semi-classical quantum theorist would mean by the determination of the velocity of an electron in an atom, no more than the number which he would have to assign in order that when he substituted that number in certain formulae and performed certain mathematical calculations he would be led to certain other numbers which he would associate with actually measurable quantities and concerning whose measurement there would be no mystery.

I will admit that some of the remarks just made have but little constructive bearing and can serve but little useful purpose other than the removal of obstructions from the mind in contemplating what is really essential. It is not my intention to imply a belief in determinism as a *practical* goal, but simply to crystallize to some extent the questions which really should enter into consideration in discussing that

question. I suppose that, in the most empirical sense, one could maintain that any finite number of events extending over a time however large could be correlated in a deterministic scheme covering that period, in manner analogous to the fact that, in a graph, it is always possible to draw a smooth curve through a finite number of isolated points.

DETERMINISM IN RELATION TO THE WAVE FUNCTION.

It would be very clarifying to my own mind if I could have firmly settled a question which resolves itself into two attitudes which I would deem at any rate worthy of contemplation in respect to the quantum theory. In the first we picture a framework of wave theory which, ideally at any rate, is completely deterministic for the waves themselves in the sense that, if some function or functions, such as the ψ function, of very many variables and applicable to the universe as a whole has specified for it at some instant appropriate initial and boundary conditions, the equations will perpetuate for all time in unique fashion the story of that function or those functions. Upon this perfectly continuous background we superpose a correlation with our experiments and all the macroscopic elements pertaining to our measurements. In imposing this correlation there arise from the nature of the correlation itself elements of uncertainty. To cite again the case of an electron, although, being an atomic entity, it does not represent a very good example, it is because of the way in which we correlate probability of position and momentum to the wave structure that there arises a principle of uncertainty which makes momentum indefinite when position is definite, and vice versa. It is in the spirit of such a continuous background that I would view Dirac's calculation of the spontaneous probability transitions of an atom in the light of a perturbation rather than simply as a postulate of a certain transition from one state to another in which the functions appropriate to one state dissolve mysteriously and are replaced by an equally mysterious birth of the functions for the new state. Such a scheme of formulation, if successful, would not be one in which indefiniteness of the correlation of the future and past in respect of the measured quantities increased continually with time; for, the

correlation at any instant would involve only the uncertainties of that instant alone.

A second possibility in thought is in the spirit of a condition in which we contemplate the universe, or a part thereof, as specified at each instant by a certain state, or in practice by a state which can be regarded as made up of a number of sub-states (the sub-states of the individual atoms, for example) and by which, as the result of our experiments, we cause some of these states to change to other states. In this process our experiments are defined as perturbations which, through the usual analytical processes appropriate to the equations which govern the ψ functions, for example, lead to a formal expression—let us say, for ψ, in terms of an expansion involving the various possible states which may be assumed, and with coefficients which determine probabilities that the state shall be assumed. The strict disciple of the quantum theory will demand that we add the words "provided that an experiment be performed" to those which speak of the probability of a state being assumed. However, we can always admit the experiment as part of the perturbation, so that again we arrive at expansions in terms of the functions for the possible states to be assumed with coefficients which determine the probabilities that the state shall be assumed. It would, of course, be in the spirit of the first formulation that the development of a state was definite and came about through the automatic dwindling to zero, with time, of those coefficients of the "eigen" function which were not the "eigen" functions of the state finally assumed. It is in the spirit of the second formulation that the transition shall be abrupt with the complete annihilation of the older era. I will admit that I find logical difficulty in this matter. Possibly it arises from ignorance and possibly a part of it is inherent in the subject. In either case, I, for one, would welcome light.

I may say that many years ago,[9] when developing the consequences of Professor Slater's invention of virtual oscillators,[10] I was led, as was also Professor Slater about the same time, to consider the consequences of the assumption that

[9] W. F. G. Swann, "The Trend of Thought in Physics," *Science*, 61, Nos. 1582, 1583, pp. 425–435, 452–460, 1925.

[10] N. Bohr, H. A. Kramers, and J. C. Slater, *Phil. Mag.*, 47, 785–802, 1924.

quanta were particles following the lines of a Poynting flux. In order to develop this idea to consistency with the facts, however, it was necessary to regard each quantum transition as the initiator of an act resulting in an electromagnetic field which obeyed all the laws of electrodynamics including interaction of the virtual oscillators themselves, and which was associated with an independent set of Polynting fluxes which guided the quanta from the seat of the original transition. It was necessary to regard different transitions as having Poynting fluxes which crossed; but, in the crossing each Poynting flux refused, as it were, to diffuse into the other. A quantum saw only its own channel. Laws for probability of transitions produced by the quanta were formulated; but, when a new transition occurred a new quantum field was created. The old quantum field had no longer any use for existence. As to whether it stayed on or not was a matter of no concern. No quantum could ever get into it to follow its paths and there was no longer a quantum in its paths to get out. It was like a field without one lone sheep. I wonder whether the spirit of the second formulation I have cited would maintain that there is anything like this in modern quantum theory.

A NEW TYPE OF FORMULATION.

Perhaps, while wandering in the orchard of fruits, some of which are bitter to the taste, and some which, while sweet, contain elements of bitterness, one may be permitted to contemplate in imagination a fruit which itself would have been sweet to one's own tongue had it existed. And so, I would like to think of the whole universe as a succession of states in which the transitions from one state to other states are called experiments of various kinds, and in which to each experiment there corresponds a mathematical operator which is analogous to the experiment in that it produces one state from another. Thus, for example, I can produce the function $\cos x$ from $\sin x$ by the operator d/dx. If I extrapolate this idea into the realm of humor and perhaps even into the realm of nonsense, I can conceive a patient with appendicitis as representative of a being defined by a set of states. I can imagine him cured as represented by another set of states, and I can imagine a mathematical operator which, when applied

to the functions of the first state, would convert them into those of the second. This operation I deem analogous to the operation performed by the surgeon and his staff. I can even meditate upon its being converted into factors representative of—in the sense of a one to one correspondence—the different sub-steps of the operation, the administrations of the nurse, the production of anæsthesia, and the activities of the surgeon's knife. However, into these realms of speculation I will not further enter, lest the complaint appropriate to your speaker be diagnosed by you as one suitable for a different kind of hospital from that in which appendicitis operations are performed.

POLAR MOLECULES IN THE EARTH'S ELECTRIC FIELD AS ABSORBERS OF WIRELESS WAVES.

BY

M. C. HOLMES, Sc.D.,

Department of Physics, West Virginia University.

The effect of the earth's magnetic field in modifying the propagation of wireless waves around the earth has long been known. The purpose of this paper is to describe some curious absorption effects to be expected as a result of the presence of water molecules, with their permanent electric dipoles, in the electric field of the earth.

The earth's electric field has been measured at various times and places over the earth's surface and found to have a mean value of about 150 volts per meter and in such a direction as to urge positive charges downward. Observed values vary considerably, being low usually near the equator and increasing as one goes poleward, reaching a value of 400 volts per meter and more in the temperate zones, thence decreasing again to 80 volts per meter and less in the polar regions, sometimes decreasing to zero and occasionally even reversing. It also exhibits daily, yearly and secular variations.[1] The strength of the field decreases with altitude and is usually considered negligible at heights above 10 kilometers. Measurements exist, however, showing cases in which it increased again above the 10 kilometer mark.[2]

When water molecules are placed in such a field, some highly interesting consequences follow. Water molecules are somewhat unique in that the centers of gravity of the positive and negative charges do not coincide, one end of the molecule being charged negatively and the other end positively.[3] Such molecules, in the electric field, therefore, will

[1] See article by W. F. G. Swann, "Atmospheric Electricity," Ency. Brit., 14th Ed.

[2] See Idrac., P., "Registrations of the Atmospheric Field at a Height of Twenty Kilometers," *Comptes Rendus*, 182, p. 1643, June 28, 1926.

[3] See P. Debye, "Polar Molecules," Chem. Cat. Co., 1929.

experience a torque tending to bring about a vertical alignment of the molecules, with the negative ends uppermost and the positive ends downward. These molecules, if displaced from the vertical position, will oscillate much like torsional pendulums, except for the important difference that they are capable of emitting and absorbing electromagnetic radiations. The natural frequency of such oscillators can be calculated using the well known equation from physics,

$$f = \frac{1}{2\pi}\sqrt{\frac{L/\theta}{I}}. \qquad (1)$$

Here, f = frequency
I = moment of inertia of the water molecule, the value of which may be taken as 2.8×10^{-40}
L = the restoring torque for an angle θ.

The equation is exactly true, of course, only for small values of θ.

Now the ratio L/θ depends upon the value of the electric field E and upon the electric moment of the water molecule, m, which is known to be approximately 1.85×10^{-18} E.S.U. It is equal to Em. Upon substituting these values in equation (1) the frequency of the oscillations is found to be

$$f = 1.3 \times 10^{10} \times E^{\frac{1}{2}}. \qquad (2)$$

This is the natural frequency of the dipole oscillators and is seen to depend only upon the magnitude of the earth's electric field E. Hence, since E ranges from 150 volts per meter, or 5×10^{-3} E.S.U., at the earth's surface to perhaps 10^{-4} E.S.U. at a height of 6 kilometers, the natural frequency of the oscillators will vary from 9×10^{8} at the surface to 1.3×10^{8} at a height of 6 kilometers. These numbers correspond to wave-lengths of 0.33 and 2.3 meters respectively, which are seen to be in the region of short wireless waves. Such oscillators are capable of absorbing strongly radiations of frequencies equal to their own natural frequencies, so that wireless waves passing through regions containing water molecules in the earth's electric field may be expected to suffer selective absorption wherever the field is of such value as to impart to the dipoles a natural frequency equal to that

of the wireless waves. For the absorption of 10 meter waves, for example, the field E would need to be only 5.3×10^{-6} E.S.U., a value which might be encountered at a height somewhere between 7 and 10 kilometers. Shorter waves would experience absorption below this level and longer waves above it. Thus, for each wave-length there will exist a level, or layer, of maximum absorption. Of course, the height of such a layer, as well as the amount of absorption, for any given wave-length will be modified by the collision frequency of the air molecules as well as by the relative abundance of water molecules. According to tables given in Humphrey's "Physics of the Air" the number of water molecules per cubic centimeter present in the air ranges from about 3×10^{17} at the surface to as much as 10^{12} at a height of 70 kilometers. The collision frequency depends upon the atmospheric pressure as well as upon the temperature. It ranges from an average value of approximately 5×10^{9} per second near the surface to perhaps 5×10^{4} per second at heights of 70 kilometers. The collision frequency near the surface is thus seen to be of the same order of magnitude as the natural frequency of the oscillators so that considerable interference may be expected there. It must be remembered, however, that 5×10^{9} is the average frequency of collision. Many individual molecules out of the 3×10^{17} present will exist between collisions for periods of time considerably greater than the average value; enough for appreciable absorption to take place. In the higher layers the collision frequency will be less and conditions will be more favorable for absorption.

The effect of the thermal motions of the air molecules in disturbing the alignments of the water dipoles, especially near the surface, must also be taken into account. This effect can be estimated from the well known equation of kinetic theory which gives the average moment, $\bar{m}$, of a dipole in the direction of the field E at a temperature T.

$$\bar{m} = \frac{m^2 E}{3kT} = \frac{3.42 \times 10^{-36} \times 5 \times 10^{-3}}{3 \times 1.37 \times 10^{-16} \times 273} = 1.5 \times 10^{-25} \text{ E.S.U.}$$

This indicates a decrease in the average moment in the direction of the field from the full value of 1.85×10^{-18} per

molecule to 1.5×10^{-25}, or by a factor of roughly 10^{-7}. This is a considerable decrease, and at first glance might seem sufficient to render negligible the effects which we have been discussing. The following consideration, however, will show that such is not the case. The number of water molecules normally present at sea level being about 3×10^{17} per cubic centimeter, the total electric moment in the direction of the field, in spite of the temperature jostlings, will be $3 \times 10^{17} \times 1.5 \times 10^{-25} = 4.15 \times 10^{-8}$ E.S.U. Small as this seems, it is equivalent, nevertheless, to 2.44×10^{10} molecules per cubic centimeter, each with full value of electric moment in the direction of the field; a number sufficient to cause considerable absorption, especially in the case of long distance transmission. This seems reasonable when it is recalled that only about 10^5 electrons, or 10^9 ions, are necessary for complete reflection of wireless waves back to earth, and that each polar molecule is equivalent to almost two heavy ions as far as absorbing and radiating ability goes.

Considering the complex nature of the mechanism of absorption and the wide ranging factors, known as well as unknown, involved, no attempt will be made to calculate the absorption. The mechanism of absorption is not due to an ordinary friction force proportional to velocity, as is usually assumed in the classical theory, but depends upon, first, a building up of oscillations in the dipoles by resonance with the passing wave, and second, a passing on of the energy thus acquired to the surrounding air molecules by collision processes. Some of the unknown factors involved are, the nature of the collisions, the question of the persistence of velocity, the effect of the electric fields of the neighboring dipoles, corrections for departure from simple harmonic motion and small values of θ and finally, the unknown size of the earth's electric field as well as the distribution of water molecules at heights above ten kilometers. It is possible, of course, to take the classical equation for the absorption coefficient for the case of electric charges subjected to restoring forces as well as frictional forces at resonance, modify it slightly for our problem and show that for the most favorable conditions the absorption will be more than appreciable. The classical equation may be put in the form $k = e^2N/f_0g$,

in which e is the charge on the particle, N is the number of particles per unit volume, f_0 is the resonant frequency, and g is the friction coefficient which is usually taken as proportional to velocity and which for our purpose will be put equal to $2mF$ where F is the collision frequency. If we then take e as the charge on an electron, N equal to our reduced value, 2.44×10^{10}, $F = 10^9$, and assume that the amplitude of oscillation is small we can put an equivalent mass in for m equal to 10^5 times the mass of the electron and we get the absorption coefficient k equal to 2.8×10^{-4} for a wireless wave of frequency 10^8 cycles per second. This, of course is much larger than we need. It is sufficient to reduce the wave to approximately 37 per cent. of its initial value in going a distance of only 3,600 centimeters through such a region. It merely serves to show once more that appreciable absorption is to be expected.

An especially curious and interesting effect may be predicted by remembering that the process pictured here is quite similar to the well known phenomenon of anomalous dispersion in the visible region. The equation for the index of refraction will contain the factor $(f_0^2 - f^2)$ in the denominator. This means that if the frequency of the wireless waves be decreased so as to pass through the resonant frequency, the part of the index of refraction due to the dipoles will decrease to a minimum, suddenly change sign, rise to a maximum and then decrease again. The importance of the phenomenon, from the present point of view, lies in the fact that when waves of a given frequency travel upward into the atmosphere, they are moving toward regions of lower natural frequencies so that as they approach the level containing resonant molecules the index of refraction will decrease to a minimum, suddenly increase to a maximum and then slowly decrease again above the layer, just as in the case of anomalous dispersion. Any wave approaching such a layer of maximum refractive index would be bent in such a direction as to follow the layer which would thus tend to act as a guide around the surface of the earth, the short waves travelling around closer to the earth than the long ones. The sudden reversal of sign at the resonant region would have the effect of introducing a discontinuity into the medium of propaga-

tion, which would make possible reflection at angles perpendicular to the layer provided the region of discontinuity were small compared with the wave-length.

One more effect ought to be mentioned. Since the earth's field exhibits daily, yearly and secular variations the propagation of wireless waves around the earth ought to exhibit corresponding variations. Daily and yearly variations, of course, are a well established fact. These variations, as well as reflection and absorption, are at present attributed to the presence of free ions or electrons with densities estimated at about 10^5 for the case of the electrons and 10^9 for the ions, whereas the effect of the much more numerous and almost equally effective water dipoles is neglected. How much of such effects are due to free charges and how much to the dipoles remains to be determined. However, the existence of these effects, however small they may turn out to be, ought to be recognized. Thus, we believe we have shown that the electric field of the earth acting on the polar molecules in the earth's atmosphere, is capable of appreciable modification of the medium with respect to the propagation of wireless waves.

THE VIOLIN.[1]

BY

POUL JARNAK,

H. C. Oersteds Foundation, Royal Polytechnic College, Copenhagen.

INTRODUCTION.

The acoustical problems involved in the violin or other stringed instruments of similar construction have, for the last two centuries, interested many scientists, but it must be admitted that very little success, if any, has been gained in obtaining an explanation or a principle for the violin's acoustic action, or a method by which such instruments could be made to achieve the tonal beauty which made the violins of Stradivarius, Amati and other old Italian violin-makers so justly famous.

In the following pages I shall try to explain a point of view of the action of the resonatory body of the violin and show how a new theory or acoustical principle can be deduced. To confirm this principle, it will be followed by several analyses of sounds from instruments made accordingly. At the same time, these results will be compared with similar results on sound analyses of tones from a few famous Italian violins obtained by Professor Backhaus in Germany.[2]

THE CONSTRUCTION OF THE VIOLIN.

The present shape of the violin is the result of an empirical development which can be traced back to the earliest known periods of culture in the Orient, but it was not until the fifteenth century that the form was completed and made somewhat similar to our present viola. The quality of tones

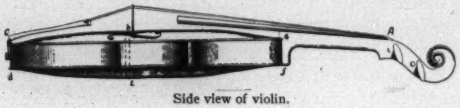
[1] "Strygeinstrumentets Resonanslegeme Betragtet Som et System af Elektrisk Koblede Svingningskredse" (*Ingenioren*, XXXIX, No. 23, p. 278, June 7, 1930), and "Violinen-Undersogelser af Resonanslegemet-Udfort paa Bell Telephone Laboratories Inc., New York" (*Ingenioren*, XLI, No. 12, p. 157, March 19, 1932).

[2] H. Backhaus, *Z. f. techn. Phys.*, 8, p. 509, 1927. *Naturwissensch.*, 17, p. 811, 1929. *Z. f. Phys.*, Bd. 62, p. 143, 1930.

in the older stringed instruments was probably very harsh and noisy; but at that time the resonatory body was made of a smaller and larger size to cover the tone register of the soprano, alto and bass voices of the singers, thereby opening the way to the great improvement of volume and mellowness achieved during the seventeenth and eighteenth centuries by the famous Italian violin makers.

The resonatory body of the violin is quite remarkable. Its usual form, shown in Fig. 1, consists mainly of the bottom

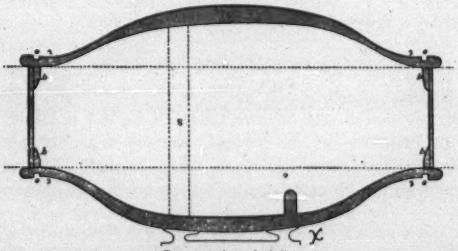
FIG. 1.



Side view of violin.

plate or back, the sides and the top plate, in which the two holes are cut like an *S*. At the upper end of the body the neck is fastened, ending with the snail, which has four screws with which the strings are tuned. In the middle, between the two sound holes in the top, is placed the bridge, over which the four strings are stretched. The other ends of the strings are fastened to the stringholder, which again is fixed to the lower end of the violin.

FIG. 2.



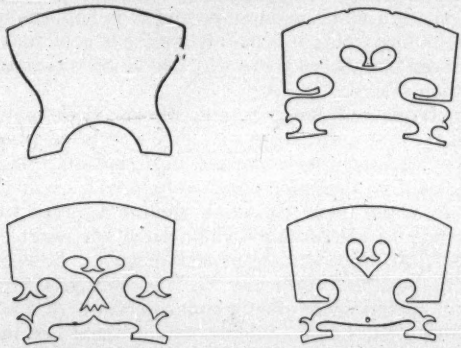
Cross section of violin.

The cross section, Fig. 2, shows the bass-bar, which runs inside the top plate about two-thirds of the full length and under the one support (x) of the bridge. This arrangement

allows the distribution of the load and the vibrations from the bridge. Near the other support of the bridge is placed the sound-post, which also carries the load from the bridge and transfers the sound vibrations to the back.

Although this construction is quite interesting, the names of the inventors of the sound-post and the bass-bar and the origin of the use of heavy wood (maple) for the back, or of light wood (spruce) for the top, etc., are not known. The construction, in these respects, was really finished before the famous Italian violin-makers endowed their violins with the beautiful tone quality not later exceeded.

FIG. 3.



Violin bridges:

Original ancient form. Antonio Amatis. Stradivarius. Nicolaus Amatis.

The Italian violin-makers brought about, in acoustical respects, a marvelous epoch in the history of the development of musical instruments, as they changed and improved the tone quality without altering the original construction. Only the bridge underwent a slight change in design in the hands of the two Amatis and Stradivarius, as is indicated in Fig. 3.

It has been an enigma ever since if such results were reached by a well-defined method, using certain acoustical principles, or merely by empirical treatment of the vibrating plates, combined with intuition and mastering an ideal aural impression of tones. As a matter of fact, the classic period of

Italian violin-making two centuries ago left us only a number of violins made by Nicolaus Amati, Guarnerius, and Stradivarius, and several others which are still highly appreciated for their pure tone quality and enormous carrying capacity of the tones.

The art of building such violins died out soon after the death of Stradivarius (1737), and the following years of violin-making have mainly consisted of copying the dimensions of the masters' violins, a method which has hardly given satisfaction. This method, however, is still used and its poor results, compared with the former creations, give a measure of the helpless position in which the technique of violin-making nowadays is placed, even with our much greater knowledge of acoustics. If a violin-maker now and then happens to make a well-sounding violin, it is mainly a piece of good luck, and it has not been possible to prove why one violin was turning out better than others.

Since those days, however, not only the violin-makers but many of the best scientists as well have worked on these problems but without much success until recently, since the methods used for detecting objectively in which way the tone quality from the famous masters' violins diverged from the tone quality of the ordinary violin were not exact enough. The electrical amplification of sound waves, however, has made this possible. Professor H. Backhaus has been very successful in obtaining photographic pictures of the sound from quite a few famous violins, among them one made by Stradivarius, so that by the help of harmonic analysis, the features of the tone quality could be seen.

The following is a translation of Professor Backhaus's conclusion of some of his results:

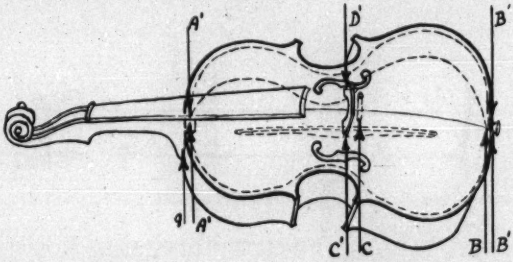
> "Some features which were noted from the sound analysis follow: Hewlett found that more sound energy was given to the lower partials on the good violins than on the poor ones. This fact was confirmed by the present investigation. Obviously, it is desired to have the timbre controlled by the fundamental if a pleasant aural impression is to be produced. If any other partial is stronger than the fundamental, the sound is sharp and less pleasant. It appeared that it was difficult, if not impossible, to pro-

duce timbres controlled by the fundamental with most of the tones from cheap violins; however, some tones from the best and most valuable violins also indicated such defects. This, of course, is often due to the material of the string."

Accordingly, it would be expected that a method of improving the tone quality should demonstrate the above statement as fully as possible. We, therefore, shall often refer to this statement during the following discussion.

After the new acoustical principle for the action of the resonatory body of my violins was developed, assisted mainly by my aural impressions and helped by the familiar acoustic laws according to Helmholtz and others, and after such violins

FIG. 4.



Top view of violin, indicating the six resonating portions.

were given practical tests at several concerts,[3] I had the privilege of making a similar tone photographic investigation of my instruments in the Bell Telephone Laboratories, New York City. It may be said here that the harmonic analysis of these oscillograms seems to confirm the statement of Professor Backhaus.

The point of view which I had found for the acoustic action of the resonatory body before applying the above-mentioned tests follows from the construction shown on Fig. 4. By the location of the bridge on the top and of the sound-post be-

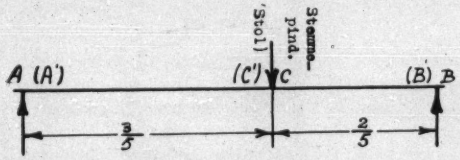
[3] K. F. W. Askoe, *Ingenioren*, XXXIX, No. 48, p. 581, November 29, 1930, and "A New Theory of Violin Building," *Musical Courier*, New York, p. 14, May 16, 1931.

tween the top and back, these plates are altogether divided into six different vibrating and mechanically coupled parts, the dimensions of which determine the quality of the sound which is transmitted from the body when the strings are excited by the violin bow.

Two of these parts belong to the bottom, as the sound-post divides it into a longer part A–C and a shorter part C–B. The other four parts are in the top. As this is glued together in the middle of two symmetrical parts, each of them is divided by the bridge at the left side into the parts A'–C' and C'–B' and at the right side into the parts A'–D' and D'–B'.

As the structure of the wood is longitudinal, the above-mentioned plates have the character of strings, and the fre-

FIG. 5.



Pressure of the sound-post upon the back of violin; and the bridge upon the left side of top plate.

quencies of the respective parts must approximately follow the usual expression for the number of vibrations of a string:

$$f_n = \frac{1}{2l}\sqrt{\frac{g \cdot P}{Q \cdot S}}, \qquad (1)$$

where l is the length of the plate parts, g the acceleration of gravity, P the tension, Q the area of the section, and S the specified weight of the wood.

From equation (1) it is seen that the frequency of the fundamental tone is decreased, if the length l, the area Q and the weight S are increased and vice versa if the tension P in the plate parts is increased.

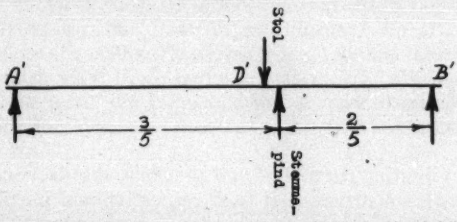
In Fig. 5 is shown the static condition caused by the pressure of the sound-post on the bottom. In the same way, the left side of the top with the bass-bar is actuated by the one support of the bridge. On Fig. 5 is further shown the right

side of the top, as this is actuated by the one support of the bridge as well as the sound-post.

From the use of two different kinds of wood for the top and bottom, thereby giving the six plate parts of the resonatory body different fundamental frequencies, it is immediately seen how this way of explaining the acoustic action of the body allows practically all the tones played on the strings to find a corresponding plate part of the resonatory body through which the sound will be transmitted to the air. This, however, brings in one important question: what is the most advantageous frequency to which each plate part has to be tuned or corresponded?

The solution of this question can be explained by referring to Fig. 5. Let us assume the back as a suspended string made,

FIG. 6.



Pressure of the bridge and sound-post upon the top plate (right side) of violin.

as it is, of wood with the total length l and cross section S, and that the sound-post is actuating at a point C at 3/5 of the length from A. The frequency of A–C will be of the reciprocal ratio to the length and, therefore, in proportion to the frequency of the part B–C, equal to $\frac{3}{2}$ because $\frac{3}{5} : \frac{2}{5} = \frac{3}{2}$. From this, it follows that the frequency of B–C is a fifth or five tones higher than the frequency of A–C. Let us combine this with the fact that the whole length A–B will vibrate with a given frequency, and assume that the point C is moved 1/20 of the whole length against A; then it is evident that if the whole length A–B has a certain frequency (for example, the fundamental tone G equals 98 vibrations per second), then the part A–C will have the frequency equal to approximately 196

vibrations per second, or the octave to G; and further, the part B–C will have the frequency d' equal to 294 vibrations per second or the twelfth to G.

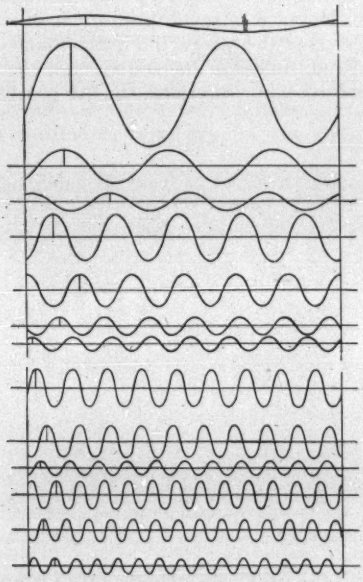
This way of explaining the best frequency distribution is quite possible and is also consistent with the mathematical investigation of the possible vibrations of a string made by Daniel Bernoulli. He found that the string vibrates, besides its fundamental tone, with a number of partials or overtones (Helmholtz), which number of vibrations equals the number of vibrations of the fundamental tone times the numbers 1, 2, 3. . . .

In dividing the resonatory body as indicated, not only the fundamental tone of the vibrating string but, at the same time also, its 2nd partial or the octave to the fundamental will be transmitted by one part and the 3rd partial or twelfth by another part of the bottom. Or at the time at which the two plate parts are transmitting the 2nd and 3rd partial, the fundamental may be created as a beat frequency between these two parts. In both cases, the actual result is the same and in contradiction to what would happen at any other division of the resonatory body (ratio between the length of the plate parts).

The vibrating parts A–C and B–C may also be considered individually as strings, and their respective 2nd partials will correspond to the 4th and 6th partial of the fundamental of the string vibration. On the other hand, the 5th partial of the string vibration, which is a third in the natural chord from the string, will not be able to find resonance in any of these plate parts, and if this should happen, the only possibility is as a partial to the vibration of the whole length of the back. Now, if this should be the case, the 5th partial of that "string" A–B would require a node for every 1/5 of its length. Therefore, one node will be located near the point C, where the sound-post is placed. This, however, is not likely to happen, because the vibrations from the string are transferred to the back at this spot, and it is, therefore, important that this transmission of energy be not hindered by the tendency of holding the "string" A–B at rest at this point (the creation of a node here) such as would be required if the 5th partial should be transmitted (amplified).

As the frequency of the back, as mentioned above, is low, around 98 vibrations per second, only the lower tones will find resonance here. It is therefore the mission of the top to give resonance to the other and higher tones which can be played on the violin.

FIG. 7.



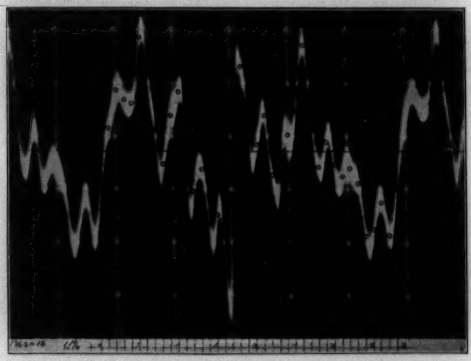
Harmonic analysis of sound from open g-string on violin, 196 vib. per second. Elementary sine-waves of $1'$–$11'$ and $13'$–$15'$ partials.

In applying the above acoustical principle to the parts of the top and going up a fifth, the plates in the one-half with the bass-bar will then have the frequencies $a' = 440$ and $e'' = 660$ vibrations per second, and the corresponding other parts of half the top will have the frequencies $h'' = 980$ and f'''-sharp $= 1470$ vibrations per second. (This notation with primes indicates to which octave the specified note belongs. For example, the note is a', then it belongs in the octave from

$c' = 260$ vibrations per second to $c'' = 520$ vibrations per second. If the note belongs to the octave below this, it is given without primes; for example, $a = 220$ vibrations per second. The next lower octave has the primes at the base of the letter; for example, $a_1 = 110$ vibrations per second, etc.)

Now, by tuning the respective pair of combined plate parts in tones following each other with intervals of five tones or a fifth, the build-up of the 5th partial in the sound of the played tones is hindered, unless in the sound from the lower tones. For example, if $G = 98$ vibrations, the 5th partial is $h' = 490$

FIG. 8.



Enlarged oscillogram (19) of open *g*-string on violin No. 1 with axis and ordinates.

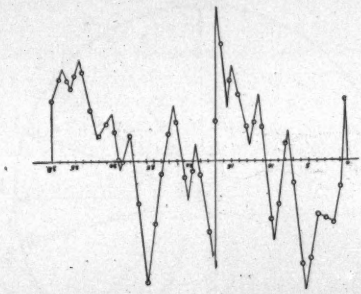
vibrations, and this will consequently find some resonance in the part (B'–C') of the top, as the frequency for resonance here is $a' = 440$ vibrations per second.

From this it follows that tones played on the violin, of which the 5th partials are higher than the frequency of the highest tuned plate part (in this case f'''-sharp = 1470 vibrations per second), will not have the 5th partial amplified. As a whole, it is to be seen that the above-mentioned principle for tuning the resonance body will permit the 5th partial to be suppressed in the sound of the tones from the two highest strings a' and e''. The resonance from the top on the other

side makes it impossible to avoid the 5th partial in tones played on the lower strings.

The above are the fundamental facts concerning the constitution of the sound from the violin. This theory is satisfactorily confirmed by the harmonic analysis of actual oscillograms. Such oscillograms were taken at the Bell Telephone Laboratories and analyzed with the aid of Pollak's tables. The results are graphically shown in Fig. 7, where the elementary sine-waves or the fundamental with its partials are given, with their phase relations. In Fig. 8 is shown the enlarged oscillogram on which the analysis is based, and on which the 38 ordinates necessary for the computation are

FIG. 9.



Curve based on addition of the first 15th computed forms from Fig. 7.

traced, together with the axis. For checking this method, a curve is drawn in Fig. 9, based on the addition of the first fifteen computed forms without the twelfth, whose amplitude is negligible. It is easily seen that this agrees very well with the original curve of the oscillogram.

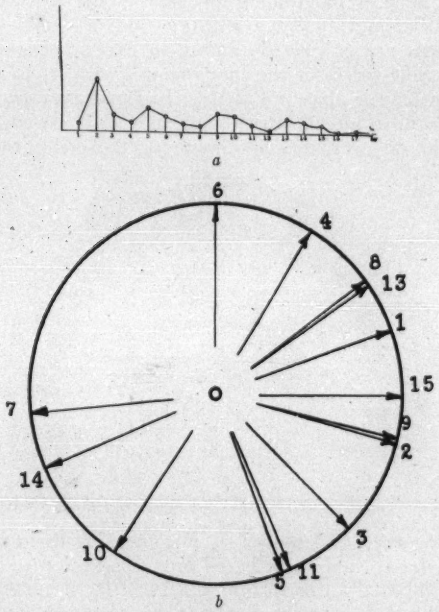
In Fig. 10 is drawn an amplitude diagram where all the amplitudes of the first eighteen partials are shown. The following section will explain what can be found in the action of the resonatory body.

EXPERIMENTS WITH STRINGED INSTRUMENTS.

The purpose of the experiments and of using harmonic analysis to explain the oscillograms was to indicate the influ-

ence of the action of the resonance body on the structure of the transmitted sound, as this would change the difference in the amplitudes of the partials and indicate whether or not the plates are tuned according to the theory developed here.

FIG. 10.



a) Relative amplitudes of vibrations of open g-string on experimental violin No. 1. *b*) Faredia-gram. The Face-angles between the partials.

If, at the same time, it is possible to prove that a similar structure is found in the sound from the famous master violins, which, of course, does not mean that the latter do not contain other valuable features, then this will emphasize the æsthetic importance of the facts here mentioned.

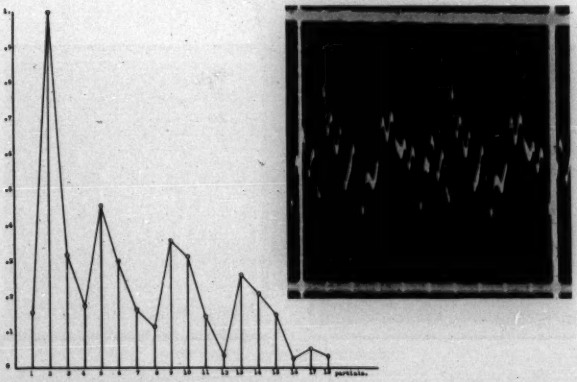
The results given here will concern only a few of the most important experiments of the quite extensive tests conducted.

Further, only the harmonic analysis of tones from the four open strings of the violin, which are tuned in the fifth g–d'–a'–e'', will be given, as these give enough data to determine the distribution of the resonance among the different parts of the resonatory body. In Fig. 10 is shown the amplitude diagram of the open g-string on violin No. 1, which, in respect to tone quality, was the best I have made.

This diagnosis indicates that the amplitude of the fundamental is very small compared with the amplitude of the 2nd partial, the octave. Further, the 3rd and 4th partials are smaller and the 5th and 6th partials are somewhat greater, and so forth. If, at the same time, it is remembered that the number of vibrations of a harmonic is so many times greater than the number of vibrations of the fundamental, as is indicated by the number of the partial, then this is a convenient way of expressing the results of an analysis from a given sound.

The amplitude diagrams of the four open strings are shown in Fig. 11. Regarding the g- and the e''-string (the last one was, in all the experiments, of steel) only a single analysis is shown, where several analyses are given of the d'- and a'-string.

FIG. 11.



Relative amplitudes of vibrations of experimental violin No. 1. Open g-string, 196 vib. (19). Open d'-string, 294 vib. (20) full line, (20a) and (20b). Open a'-string, 440 vib. (21) (upper curve). Open e''-string, 660 vib. (22).

Fig. 11 continued on pages 232 and 233.

FIG. 11.—*Continued.*

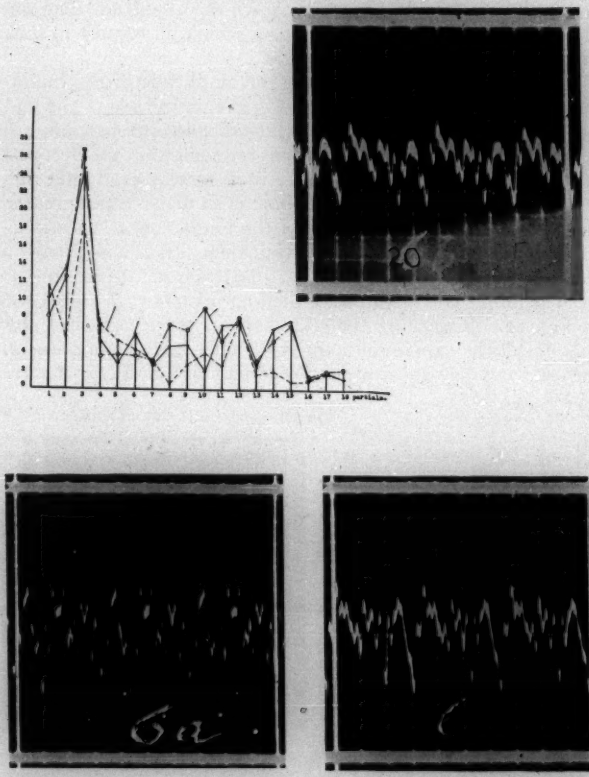
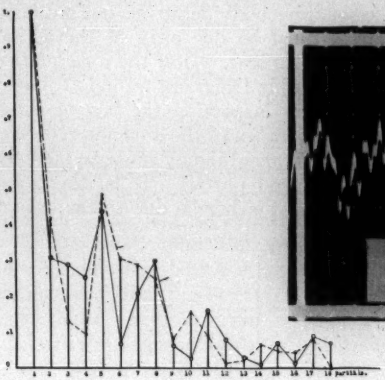
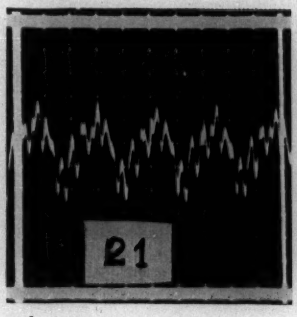
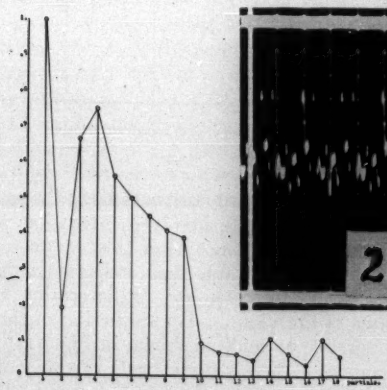
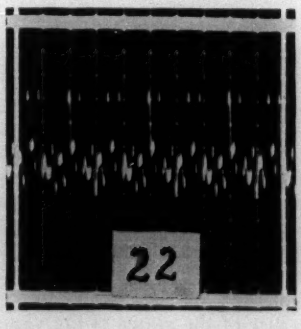


FIG. 11.—*Continued.*



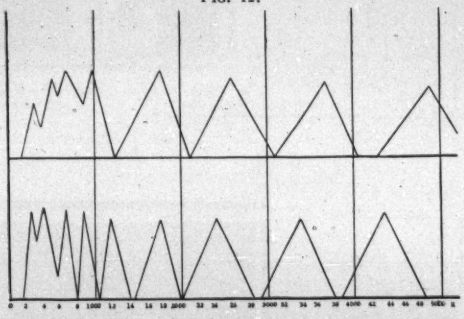






The form of the oscillogram from these two strings was quite interesting and remarkable and indicated a variation of the tone energy; that is, a variation of the fundamental and the partials as well. The resonatory body, however, has not been changed; so this variation must be due only to the action between the string and the violin bow. How the tone quality is changed can easily be followed in Diagrams 20 and 21, but it is not possible to ascertain by inspection the law of these changes. The analysis, for example, of the *d'*-string (No. 20) indicates that a gain in the fundamental may cause an increase as well as a decrease of the 2nd partial. It is, therefore, obvious that these changes must either be due to the action

FIG. 12.



Location of resonance in good (No. 1) and poor (No. 2) experimental violins.

between the string and the violin bow or to the reaction from the resonatory body or presumably to both actions at the same time. As this question seems quite involved, attention will merely be called here to this relation. After this, we proceed with pointing out that the diagrams also indicate certain minima and maxima which we can use to determine the resonance of the plates of the violin. By inspection of these four diagrams, it is possible to find, as shown in Fig. 12, the location of the resonance of the six-plate parts of the violin inside the limit of about 2000 vibrations per second. The location of resonance found in the corresponding amplitude diagram from another experimental violin (No. 2), not so good,

is also given in Fig. 12. Comparison shows that a resonance located around 392 vibrations per second is not found in violin No. 2, and further, that the number of vibrations of its plates is higher than that found in the plates of the good violin.

An investigation of these oscillograms from the two violins proves the basic assumption that the resonatory body consists of six different tuned plate parts.

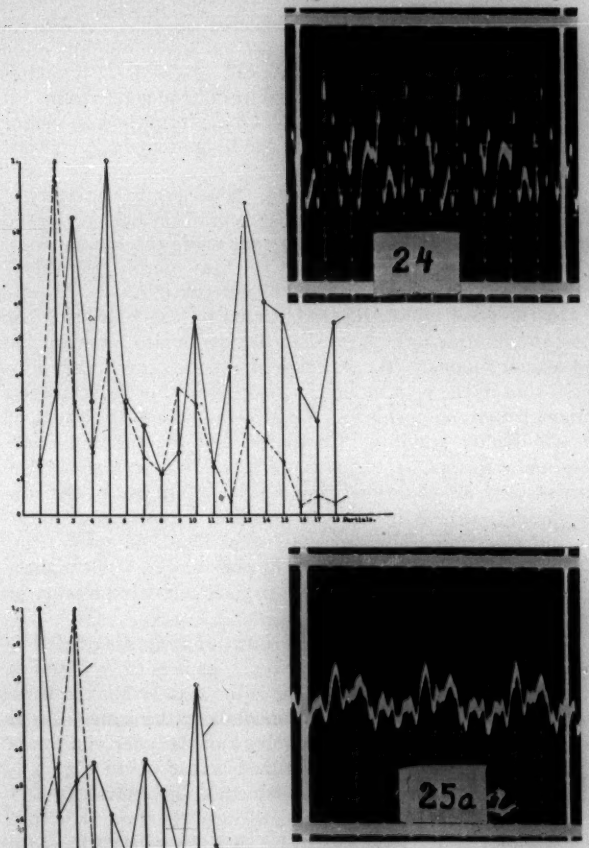
Now, let us compare, in Fig. 13, the structure or quality of the same tone in the amplitude diagrams of the open strings of the two violins, and we shall see that while the fundamental from the open *g*-string has about the same amplitude, this is far from being the case with the higher partials.

In the good violin, the 2nd partial on the *g*-string is the most dominating, which is what Backhaus also found in the old master violins. My experiment, on the other hand, indicated that in the poor violin the 5th partial was the strongest, although a strong part is also found around the 12th to 18th or at still higher partials. Compared to this, the amplitude diagram of the open *d'*-string is much better, even if the higher partials here are somewhat pronounced, compared to the corresponding partials in the good violin. Comparison of the *a'*- and *e''*-strings shows, on the other hand, no noteworthy difference between the good and the poor violin. The highest strings in both violins gave also a good aural impression of tone quality.

The difference in the two experimental violins is related to the dimensions of the resonatory body, as it is to be found in the back of the poor violin. This is too thin or has too little mass; the top in both violins is tuned about the same. From this, it follows that, provided the back of the poor violin was replaced by a heavier one and tuned according to Fig. 5, a violin of a much better tone quality could be obtained.

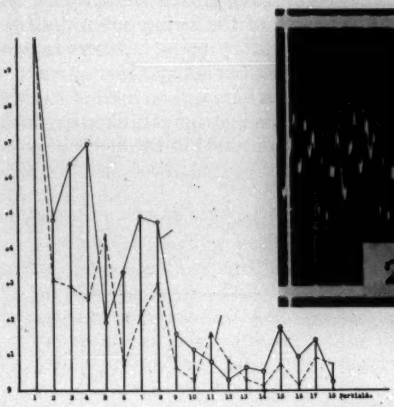
The amplitude diagrams indicate, also, the same structure as was found by Backhaus; namely, that the sound is built up of a far higher number of partials than were earlier found. My experiments explain further the fact found by Backhaus, that it is because of the tuning of the resonatory body that one does not succeed in getting a greater fundamental on the open *g*-string, as is often the case.

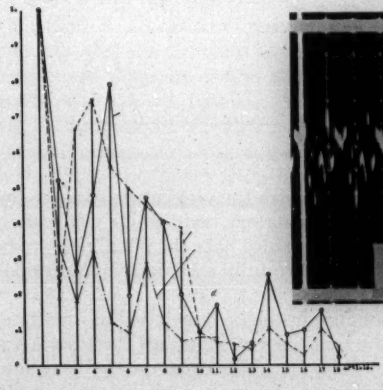
FIG. 13.



Relative amplitudes of vibrations. The full drawn curves refer to experimental violin No. 2; the dotted curves to experimental violin No. 1. Open g-string, 196 vib. (19, 24). Open d'-string, 294 vib. (20, 25a). Open a'-string, 440 vib. (21, 26). Open e''-string, 660 vib. (22, 27). Storioni-violin, dash and dot (Backhaus).

FIG. 13.—*Continued.*

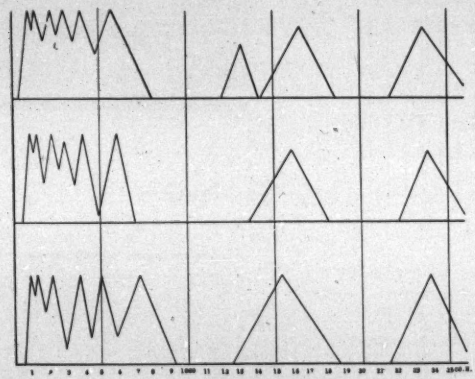




What is said here about the resonatory body of the violin concerns also the viola, the violoncello, and the double bass. As the resonatory body on those instruments must give resonance for lower tones, it should have greater dimensions, and it is an old rule that the length of the string determines the approximate length of the resonatory body. Otherwise, the construction is similar to that of the violin in Fig. 4.

The experimental results given here will be limited to those obtained with a violoncello. This instrument, like the violin, has four open strings, but they are tuned in the fifth *C–G–d–a*.

FIG. 14.



Effect of position of the bridge upon the body resonance of the experimental 'cello.

Three experiments were made with the violoncello to show the change in the tuning of the resonatory body which was caused by moving the supports of the bridge near the pin about 3 mm. against the latter. This is more fully explained by examination of Fig. 6.

The location of resonance in the body is based on the three sets of analyses from the four open strings, and the results are combined, as shown in Fig. 14. The trace on the base line indicates the location before the bridge was moved; that in the middle the condition after the move, and the upper trace the location of resonance after the bridge was moved back to its

proper place, according to the aural impression of the most mellow and pleasing sound.

A glance at Fig. 6 will show that the tension of the concerned plate parts of the top is decreased; consequently, according to equation (1), the number of vibrations should be smaller after the move. This is confirmed by the distribution of resonance in Fig. 14, which indicates that moving the bridge decreases the respective vibrations of the three highest tuned plate parts. This is especially to be found at the two highest tuned plate parts A'–D' and D'–B'; see Fig. 6, which number of vibrations is about 100 vibrations per second lower than before the move.

Before the third oscillograph record was taken, all the plate parts of the resonatory body were adjusted by moving the bridge and sound-post until the tone quality most pleasant to the ear was obtained. This adjustment is the last made before an instrument leaves the violin-maker.

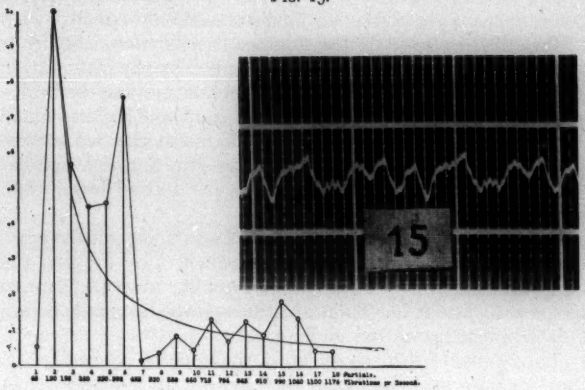
The results of this are shown in the upper trace in Fig. 14. They indicate an even better distribution of resonance than before. Further, the amplitude diagrams of the four open strings, according to this last condition, are given in Fig. 15. It is shown in the open *G*-string (= 65 vibrations per second) that there is a location of resonance of the top which amplifies the 5th and the 6th partials. On the other hand, the last amplitude diagram of the open *a*-string indicates the suppression of the 5th partial. When we add to this picture the slight decrease in amplitudes in the open *G*-string and the open *D*-string, then the suppression of the 5th partial of the sound from the higher tones is a proof of the perfect tuning of the resonatory body. At the same time, this permits the tone quality to be primarily dominated by its greatest fundamental; also the matching of lower partials which give the tonal beauty and a well-balanced acoustical impression as a whole.

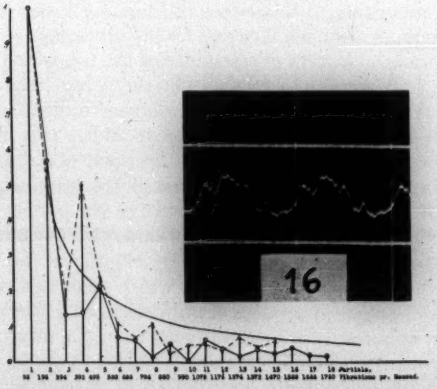
That this result is more obvious from the analysis of the tones from the violoncello than from the best experimental violin is due to its better distribution of resonance.

For checking the oscillographic record, the tone from the open *G*-string was also analyzed with the electrical analyzer [4]

[4] R. L. Wegel and C. R. Moore, *Bell Syst. Techn. Journal*, 3, 1924, p. 299.

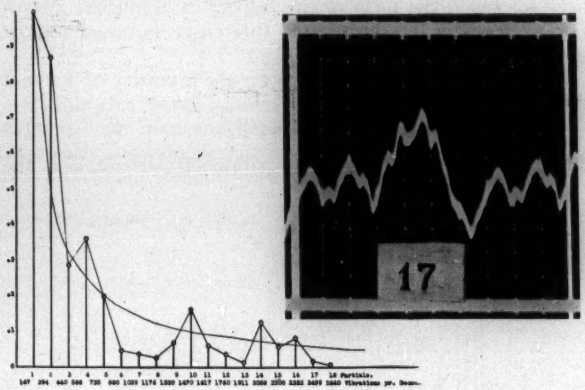
FIG. 15.

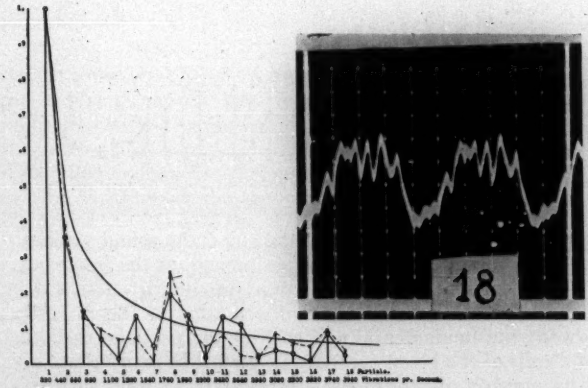




Violoncello. Open c_1-string, 65 vib. (15). Open G_1-string, 98 vib. (16). Open d-string, 147 vib. (17). Open a-string, 220 vib. (18).

FIG. 15.—*Continued.*





by Wegel and Moore. The dotted curve in the amplitude diagram indicates that this test was satisfactory.

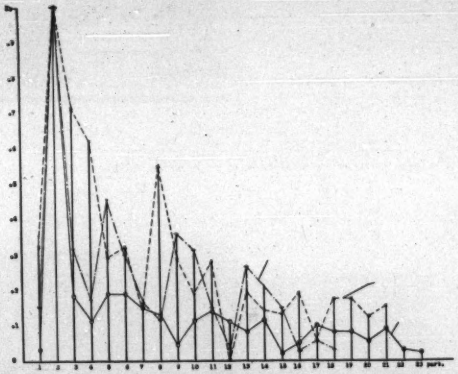
In each of the last diagrams there is drawn a hyperbolic curve by which the form of the respective amplitude curves may be compared. The use of this curve is based on the following assumption:

Let us consider the expression for the intensity of a sound vibration

$$J = \tfrac{1}{2}SC(\omega a')^2, \qquad (2)$$

where a' is the amplitude of the vibrating particle, S is the

FIG. 16.



Relative amplitudes of vibrations of open g-string of violins: Stradivarius, full line; Carlo Bergonzi, dotted line: (Backhaus); experimental violin No. 1—dash and dot.

average value of the density of the air, C the sound velocity, and ω the number of vibrations per second, or the frequency.

Assuming that this sound is solved into its partials and that we have a constant and equal distribution of the intensity between the fundamental and each of the partials, then the amplitude a' of a given partial must be just so many times less as its number of vibrations is increased. A tone with this feature we shall call an ideal sound.

The intensity of an ideal sound of a given tone can, therefore, be expressed solely by the number of vibrations of the

fundamental and its amplitude a', as follows:

$$J_i = \tfrac{1}{2}SC\left[(\omega_1 a_1')^2 + \left(2\omega_1 \frac{a_1'}{2}\right)^2 + \left(3\omega_1 \frac{a_1'}{3}\right)^2 + \cdots + \left(n\omega_1 \frac{a_1'}{n}\right)^2 + \cdots\right]. \quad (3)$$

If this ideal sound is used as a kind of unit for comparing an equal tone from another instrument, then this can be expressed by some coefficient of partials. Thus, the new tone with an arbitrary sound feature can be written:

$$J_a = \tfrac{1}{2}SC[(\omega_1 a_1')^2 + (2\omega_1 a_2')^2 + (3\omega_1 a_3')^2 + \cdots + (n\omega_1 a_n')^2 + \cdots] \quad (4)$$

and as $a_n' = k_n \frac{a_1'}{n}$ we have

$$J_a = \tfrac{1}{2}SC\left[(\omega_1 a_1')^2 + \left(2\omega_1 k_2 \frac{a_1'}{2}\right)^2 + \left(3\omega_1 k_3 \frac{a_3'}{3}\right)^2 + \cdots + \left(n\omega_1 k_n \frac{a_1'}{n}\right)^2 + \cdots\right]$$

$$= \tfrac{1}{2}SC(\omega_1 a_1')^2[1 + k_2^2 + k_3^2 + \cdots + k_n^2 + \cdots]. \quad (5)$$

According to the way this expression (5) is deduced, it must not be considered as an expression to which numerical value can be given. As any tone, however, can be decomposed into its fundamental and partials, this coefficient k_n will give just the ratio of the component of the sound tested to the corresponding component of the ideal sound; it will therefore be easy to discriminate the tone quality and thereby to obtain a picture of the value of the violin.

Professor Vladimir Karapetoff of Cornell University, who himself has experimented with stringed instruments and developed a five-stringed cello,[5] has authorized the following statement:

> "I am a proud owner of one of Mr. Jarnak's cellos, and have also used another cello and a double bass of his in public recitals. His instruments possess a rich and mellow

[5] Vladimir Karapetoff, *The Journal of the Franklin Institute*, 207, p. 645, May 1929.

tone unusual in 'green' violins and cello; their carrying tone quality, with a slight pressure of the bow, is indicative of a ready response of the resonating body. His contribution to the science and art of stringed instruments is of great importance; the same acoustical principle can probably be extended to other groups of musical instruments."

CONCLUSION.

As a result of a series of theoretical developments and experiments, I have succeeded in building some superior stringed instruments. These instruments give tones exceedingly rich, mellow, and pleasing to the ear; and a comparison of oscillograms of these tones with those obtained by Backhaus on old Italian instruments indicates that the tone quality, as far as it can be measured in the oscillograms, is very near to that of the old masters' violins.

This investigation may be pursued even further, but it is my hope that the application given here may revive the art of violin building.

Further, a method for the objective appreciation of new types or new forms for the resonatory body is given which may open the way for subsequent scientific work on other forms or materials designed to produce even greater volume and more mellow tone quality.

No attempt will be made here to discuss these problems. It may only be suggested that the development here given may result in the five-stringed violoncello,[6] used at an earlier epoch, again coming into use. Johann Sebastian Bach (1685–1750) has composed for it, and several violin-makers have since tried to introduce it, as the added fifth string to a considerable extent facilitates the playing. Recently, Professor Vladimir Karapetoff has taken the problem under consideration and has investigated what material and dimensions can best be used for the string. The final solution seems now only to require a satisfactory, strongly constructed and well-tuned resonatory body.

Finally, it may be said that it is much easier to produce tones with the bow in my improved violins and that the sound has a greater carrying capacity and will even fill great concert halls. This is evidently caused by the better efficiency in the

energy transformation between the vibrating string and the resonance body and by the resulting possibility of improved emission of sound, as is explained above.

For the courtesy of the Bell Laboratories and for the assistance of John C. Steinberg, Daniel W. Farnsworth and Arthur Meyer in recording the tones with the new rapid oscillograph, I wish to express my thanks. I also wish to thank E. H. Mynster and W. Marstrand for their help and suggestions.

The financial assistance for the harmonic analysis of this investigation has been granted by the trustee of the H. C. Oersted Foundation.

Mechanical Cotton Picker.—There has been much discussion in recent years relative to unemployment and insecurity due to inventions of labor-saving machinery. In this connection a report by the Works Progress Administration on the mechanical cotton picker is of interest. It states that many uncertainties stand in the way of an attempt to evaluate the effects of a successful mechanical cotton picker on the labor economy of the Cotton Belt. Estimates based on a single-row machine of a type similar to those now being used experimentally show that total costs and losses for mechanical picking would come to $23.66 per bale, or $14.84 per acre, the equivalent of $1.65 per 100 pounds of seed cotton. This may be contrasted with the rate of about $1.00 per 100 pounds paid for hand picking in the Mississippi Delta in 1936. An improved two-row, twice-over machine having a longer life and requiring only one operator might lower the cost of mechanical picking to around 65 cents per 100 pounds. Several years will be required to develop such a machine. In the meantime the picker is not likely to take the Cotton Belt by storm. Successful one- and two-row machines, if developed, might, in the course of perhaps 10 years, be applied to an upper limit of about half the present cotton acreage and displace a maximum of 2 million hand pickers for the picking season of about 40 workdays. The displacement of half a million pickers within a period of 5 to 10 years appears more probable, however. Although such a displacement might not add appreciably to the total volume of unemployment for the nation as a whole, it would tend to aggravate the insecurity of the tenants and sharecroppers in the localities where the picker was used.

R. H. O.

MATRIX-OPERATIONAL METHODS IN MECHANICAL VIBRATIONS.

BY

LOUIS A. PIPES, Ph.D.,

Alumni Research Fellow, The University of Wisconsin.

INTRODUCTION.

The purpose of this paper is to present a method of full generality for the solution of mechanical vibration problems in systems of several degrees of freedom. The method here presented is adequate for the solution of freely vibrating systems or for systems in forced vibration.

The elegance and simplicity of matrix notation and the Laplacian transform or operational method are brought out most clearly in the analysis of systems of many degrees of freedom. The treatment here presented is essentially a translation into mechanical terminology of a paper by the author dealing with a similar electrical problem (see reference no. 2). The reader not acquainted with matrix notation is referred to reference no. 1.

THE GENERAL EQUATIONS.

The Lagrangian equations of motion for a general mechanical system defined by n parameters $(x_1, x_2, \cdots x_n)$ that is executing small motions about a position of equilibrium may be written in the form:

$$\begin{aligned} Z_{11}(x_1) + Z_{12}(x_2) + \cdots + Z_{1n}(x_n) &= F_1(t), \\ \cdots\cdots\cdots\

Let us introduce the notation:

$$(3) \qquad [x] = \begin{pmatrix} x_1 \\ x_2 \\ \cdot \\ \cdot \\ x_n \end{pmatrix} \text{ the coördinate matrix,}$$

$$(4) \qquad [A] = \begin{pmatrix} A_{11} \cdots A_{1n} \\ \cdot \quad \cdot \quad \cdot \\ A_{n1} \cdots A_{nn} \end{pmatrix} \text{ the inertia matrix}$$

and two additional matrices, similarly defined:

(5) $[B]$, the damping matrix,

(6) $[C]$, the stiffness matrix,

and a columnar matrix whose elements are the applied forces:

(7) $[F(t)]$, the force matrix.

The notation here employed is that used in reference no. 3.

By means of this notation, the equation of motion, (1), may be conveniently written in the form:

$$(8) \qquad [A][\ddot{x}] + [B][\dot{x}] + [C][x] = [F(t)],$$

where the dots indicate differentiation of the elements of the matrices with respect to time.

If we now multiply this equation by $pe^{-pt}dt$, and integrate from 0 to infinity we obtain (the real part of p is supposed positive):

$$(9) \quad [A]p\int_0^\infty e^{-pt}[x]dt + [B]p\int_0^\infty e^{-pt}[x]dt + [C]p\int_0^\infty e^{-pt}[x]dt = p\int_0^\infty e^{-pt}[F(t)]dt.$$

(The reader unacquainted with the notion of integration of matrices is referred to no. 1 and no. 2.)

Let:

$$(10) \qquad [y(p)] = p\int_0^\infty e^{-pt}[x(t)]dt$$

or in briefer notation:

$$[y(p)] \doteqdot [x(t)] \tag{11}$$

and let:

$$[G(p)] = p\int_0^\infty e^{-pt}[F(t)]dt \tag{12}$$

or,

$$[G(p)] \doteqdot [F(t)]. \tag{13}$$

(14) Let $[x^0]$ be the matrix giving the initial displacements at $t = 0$.

(15) Let $[x^1]$ be the matrix giving the initial velocities at $t = 0$.

By an integration by parts, we obtain:

$$\begin{aligned} p\int_0^\infty e^{-pt}[\dot{x}]dt &= pe^{-pt}[x]\Big]_0^\infty + p^2\int_0^\infty e^{-pt}[x]dt \\ &= -p[x^0] + p[y]. \end{aligned} \tag{16}$$

Similarly:

$$p\int_0^\infty e^{-pt}[\ddot{x}]dt = -p[x^1] - p^2[x^0] + p^2[y]. \tag{17}$$

Therefore equation (9) transforms to:

$$\begin{aligned} &(p^2[A] + p[B] + [C])[y] \\ &\quad = [G] + (p^2[A] + p[B])[x^0] + p[A][x^1]. \end{aligned} \tag{18}$$

If we let:

$$Z_{ij}(p) = p^2A_{ij} + pB_{ij} + C_{ij}, \tag{19}$$

we can write:

$$[Z(p)][y] = [G] + (p^2[A] + p[B])[x^0] + p[A][x^1]. \tag{20}$$

Multiplying (20) by the inverse of $[Z(p)]$, $[Z(p)]^{-1}$ we obtain:

$$\begin{aligned} [y] = [Z(p)]^{-1}\{&[G] + (p^2[A] \\ &+ p[B])[x^0] + p[A][x^1]\} \end{aligned} \tag{21}$$

and by the Fourier-Mellin formula we have:

$$[x] = \frac{1}{2\pi j}\int_{c-j\infty}^{c+j\infty} \frac{e^{pt}[y]dp}{p}, \qquad j = \sqrt{-1} \tag{22}$$

as the explicit solution to the problem.

In the contour integral expression, the constant c is such that all the singularities of the integrand lie to the left of the path of integration.

The above formula for $[y]$ which is the operational equivalent of the coördinate matrix $[x]$, contains a great deal of information in that the effect of the impressed forces, the initial displacements, and the initial velocities is clearly evident. In the absence of impressed forces $[G(p)] = [0]$, and then we have the case of free vibrations. Since $[y]$ is the operational equivalent of the matrix $[x]$, all the technique of the Heaviside calculus is at once at our disposal.

The procedure of obtaining the explicit solution is that of calculating the sum of the residues of the integrand at its poles. Since the poles of the integrand occur at $p = 0$ and at the poles of $[Z]^{-1}$, the zeros of the determinantal equation must be located. This equation may be written in the form:

$$|Z(p)| = 0. \tag{23}$$

The location of these zeros is the most laborious part of the process. This can be most conveniently done by Graeffe's "root squaring" method. See no. 4. It may be noted that having located the poles, one does not have to laboriously solve for the integration constants in terms of the initial conditions. It is the latter process that consumes so much time and effort in the classical or even in the more recent matrix methods of analysing this type of problem.

AN ILLUSTRATIVE EXAMPLE.

To illustrate the general method, let the following simple problem be considered: A system consists of two simple pendula each having a bob of 1 pound weight. Let one pendulum be 32.2 ft. long and the other one 8.05 ft. long. Suppose the two bobs are connected together through a dash

pot in which the force varies directly as the speed between the two sides. For a speed of 1 foot per second, the force will be assumed to be one poundal.

Let:

x_1 = the displacement of the first bob,
x_2 = the displacement of the second bob.

The equations of motion are:

$$\begin{aligned} &\frac{d^2x_1}{dt^2} + \left(\frac{dx_1}{dt} - \frac{dx_2}{dt}\right) + x_1 = 0, \\ &\frac{d^2x_2}{dt^2} + \left(\frac{dx_1}{dt} - \frac{dx_2}{dt}\right) + 4x_2 = 0. \end{aligned} \tag{24}$$

Hence in our general notation:

$$[A] = \begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix}, \tag{25}$$

$$[B] = \begin{pmatrix} 1 & -1 \\ -1 & 1 \end{pmatrix}. \tag{26}$$

$$[C] = \begin{pmatrix} 1 & 0 \\ 0 & 4 \end{pmatrix}, \tag{27}$$

$$[Z(p)] = \begin{pmatrix} (p^2 + p + 1) & -p \\ -p & (p^2 + p + 4) \end{pmatrix}. \tag{28}$$

Let the initial displacements at $t = 0$ be:

$$[x^0] = \begin{pmatrix} 0 \\ .5 \end{pmatrix}. \tag{29}$$

Let the initial velocities be:

$$[x^1] = \begin{pmatrix} -.5 \\ 0 \end{pmatrix}. \tag{30}$$

Now,

$$Z(p) = p^4 + 2p^3 + 5p^2 + 4. \tag{31}$$

The roots of the determinantal equation $|Z(p)| = 0$, and hence the position of the poles can be obtained from the pair of quadratics:

$$\begin{aligned} &p^2 + 1.5p + 1.4 = 0, \\ &p^2 + .5p + 2.85 = 0 \end{aligned} \tag{32}$$

and we obtain:

$$
\begin{aligned}
p_1 &= -.75 + j.915, \\
p_2 &= -.75 - j.915, \\
p_3 &= -.25 + j1.67, \quad j = \sqrt{-1}. \\
p_4 &= -.25 - j1.67,
\end{aligned} \tag{33}
$$

These roots give us the position of the poles from which the residues of:

$$
[x] = \frac{1}{2\pi j} \int_{c-j\infty}^{c+j\infty} \frac{[y] e^{pt} dp}{p} \tag{22}
$$

may be calculated.

Carrying out the procedure expressed in (21) by routine matrix multiplication, we obtain the following expressions for x_1 and x_2.

$$
x_1 = \frac{1}{2\pi j} \int_{c-j\infty}^{c+j\infty} \frac{.5(p-1) p e^{pt} dp}{|Z(p)|}. \tag{34}
$$

$$
x_2 = \frac{1}{2\pi j} \int_{c-j\infty}^{c+j\infty} \frac{.5(p-1)(p^2+p+1) e^{pt} dp}{|Z(p)|}. \tag{35}
$$

Evaluating the residues at the poles given by (33), one readily obtains:

$$
\begin{aligned}
x_1 = &- .219 \exp(-.75t) \cos(.915t) \\
&- .797 \exp(-.75t) \sin(.915t) \\
&+ .219 \exp(-.25t) \cos(1.67t) \\
&+ .0723 \exp(-.25t) \sin(1.67t),
\end{aligned} \tag{36}
$$

$$
\begin{aligned}
x_2 = &\ .271 \exp(-.75t) \cos(.915t) \\
&+ .287 \exp(-.75t) \sin(.915t) \\
&+ .223 \exp(-.25t) \cos(1.67t) \\
&- .194 \exp(-.25t) \sin(1.67t)
\end{aligned} \tag{37}
$$

as the explicit solution of the problem.

CONCLUSION.

The simplicity and compactness of the method here presented should be evident from the example given. It may be further noted that in the cases where the determinantal equation $|Z(p)| = 0$ has multiple roots, the method proceeds in exactly the same manner as in the case of distinct roots. The only difference is that one must evaluate the residues at poles of higher order.

BIBLIOGRAPHY.

1. "Matrices in Engineering," Pipes, *Elect. Eng.*, Sept. 1937, pp. 1177–1190.
2. "Laplacian Transform Circuit Analysis," Pipes, *Phil. Mag.*, **24**, 502–510.
3. "Matrices Applied to Motions of Damped Systems," Duncan and Collar, *Phil. Mag.*, 7th series, Feb. 1935, **19**, 197–219.
4. "Mathematics of Modern Engineering," Doherty and Keller, John Wiley.

Sterilization by Radiation.—(*Heating, Piping and Air Conditioning*, Vol. 10, No. 1.) A. R. DENNINGTON in a recent talk at the Canadian Section of the American Institute of Electrical Engineers predicted the use of a sterilizing lamp for purifying the air we breathe. Since the discovery of the existence of microörganisms, attempts have been made to control them by creating environments favorable to those which it is desired to have survive. Sunlight is considered as a germicide and is effective even though both ultra-violet and infra-red rays are filtered out. Various experimenters have demonstrated that the shorter wave-lengths are more effective than the longer ones in their lethal action. For a given expenditure of radiant energy, the most effective radiation is in the region of 2537 angstroms and down to 2000 angstroms or even less. The extremely short wave-lengths are readily absorbed in the air and hence are effective only at short distances from the source. For sterilizing problems, a tubular lamp is available made of special glass and containing low pressure mercury vapor as well as a mixture of inert gasses, such as argon and neon, to reduce the starting voltages. The temperature of the tube is only a few degrees above that of the surrounding air so that it may be installed in places where any marked increase in the heat load is a disadvantage. Sterilizing lamps have been adapted to materially reduce the hazards of surgical operations, according to Mr. Dennington, and there is a possibility that this procedure may become standardized hospital practice. It was pointed out that perhaps the efficiency of office workers may be increased by not only having the air properly heated or cooled and humidified to meet health requirements, but also treated to reduce bacteria count, thus releasing additional energy for battling the unseen microscopic enemies.

R. H. O.

NOTES FROM THE NATIONAL BUREAU OF STANDARDS.*

NAVY RADIO METEOROGRAPH SYSTEM.

In Technical News Bulletin 239 (March 1937), the development of a radio meteorograph system, for use by the aerological service of the United States Navy Department, was announced. Since that time the component parts of the system have been materially improved and its performance has been determined through simultaneous radio meteograph and aerograph soundings of the upper atmosphere under typical service conditions. Fifty early morning radio meteorograph soundings were made in coöperation with the aerological and radio personnel of the Navy Department at the Naval Air Station, Anacostia, D. C. The soundings were timed to occur practically simultaneously with the regular early morning aerograph soundings in a Navy airplane. Gratifying agreement was obtained between the observations of the two types of instrument, the comparisons demonstrating conclusively that the radio meteorograph is ready for routine daily use. While affording the same order of accuracy of measurement as the aerograph, the radio meteorograph can be used during adverse visibility conditions and can attain much higher altitudes; (12 to 15 miles instead of about 4 miles for the usual airplane sounding).

The radio meteorograph attached to a small unmanned balloon, sends down radio signals which give a measure of the variations in atmospheric pressure, air temperature, and air humidity as the balloon ascends. The decrease in barometric pressure as the balloon rises is utilized to operate a small switch arm which moves over a set of electrical insulating and conducting strips. The conducting strips are electrically interconnected with two resistors which control the modulating frequency or pitch of the radio signals. One of these resistors consists of a small capillary glass tube filled with an electrolyte which varies markedly in electrical

* Communicated by the Director.

resistance with the surrounding air temperature. The second resistor is mechanically varied by the expansion or contraction of a hair element and hence varies with the relative humidity. The temperature resistor is normally in circuit so that the modulating frequency or pitch is normally proportional to the temperature. At predetermined pressure levels, corresponding to approximately 500 foot increments in the height of the balloon, the switch-arm switches in the humidity resistor and the modulating frequency or pitch becomes a measure of the relative humidity. At the ground receiving station, an automatic graphical frequency recorder connected in the output of a receiving set converts the variations in pitch into a plot of temperature and humidity against pressure.

The improvements incorporated in the system during the past year have increased the stability of the modulating oscillator and the accuracy of frequency measurement at the ground receiving station. The design of the capillary therometer has been materially improved and its cost reduced. A simplified calibrating and operating procedure has been evolved which increases the accuracy of the observations while at the same time permitting rather wide tolerances in manufacture in order to reduce cost.

The results of the comparison tests of the radio meteorograph and the aerograph showed an agreement of pressure readings within about 2 millibars (1½ millimeters of mercury), agreement of the temperature readings within 2 degrees Centigrade, and agreement of the humidity readings within 10 per cent relative humidity. One unusual comparison test is of interest. On November 9, 1937, a new air mass passed over Anacostia just after the release of the radio meteorograph. The radio meteorograph was carried away with the old air mass while the airplane sounding was in the new air mass. This was clearly shown by the comparison. Observations obtained by an airplane sounding at Norfolk about one hour earlier showed the same temperature inversion indicated by the radio meteorograph and not shown by the Anacostia aerograph record. A second radio meteorograph, released about four hours after the simultaneous test gave good agreement with the Anacostia aerograph record, indicating that its sounding was now in the new air mass.

CODE FOR PROTECTION AGAINST LIGHTNING.

The protection of persons and property against lightning is a subject of widespread interest and of considerable importance. Since the time of Benjamin Franklin the value of lightning rods for safeguarding buildings has been recognized. Early in the art of utilizing electric energy, lightning arrestors were employed in connection with exposed overhead lines. In the fields of both buildings and electric equipment there has been a great variety of practice, and many installations have been made which did not adequately fulfill the purpose for which they were intended.

A code establishing standards of practice for lightning protection, was included by the American Standards Association in the first list of safety codes on its program. The American Institute of Electrical Engineers and the Bureau were designated as sponsors for this project, the scope of which included the protection of buildings, oil tanks, and other structures; trees, livestock, and persons; and overhead electric lines and apparatus connected to them.

A sectional committee was organized by the sponsors in 1921, and in 1929 it completed the formulation of definite standards for the protection of persons (Part 1), buildings and other property (Part 2), and structures containing flammable liquids and gases (Part 3). These were published in Miscellaneous Publication M92 of the Bureau. In 1932 the sectional committee revised Parts 1 and 2 of the code, and in 1937 it made slight revisions in all three parts. The last edition was approved by the American Standards Association, October 29, 1937, and issued by the Bureau as Handbook 21. Copies are obtainable from the Superintendent of Documents, Government Printing Office, Washington, D. C., at 15¢ each.

The subject matter contemplated for this code originally comprised also two parts dealing with electric apparatus and lines. Practice in these fields had not, however, crystallized to the point where it was felt that definite standards could be set up. The committee consequently issued only a preliminary report on this part of the subject, which was published by the Bureau as Miscellaneous Publication M95.

Part 1 of the code is very brief and gives some simple

instructions on what a person should do during a thunderstorm to avoid being struck by lightning. If one cannot avoid remaining out of doors, it is well to keep away from isolated trees, from wire fences, from hilltops and wide open spaces, and from small sheds which are in an exposed location. If no building is available for shelter, a cave, a deep valley, a grove of trees, or dense woods offer some protection.

Part 2 of the code gives specifications for lightning rods to protect buildings and sets forth the requirements for satisfactory installations. Special sections are devoted to steeples, water towers, chimneys, hangars, buildings containing baled flammable materials, ships, and trees. A section is included on the protection of livestock in fields.

Part 3 deals with the protection of oil tanks and gives fundamental principles to be applied, rather than the detailed construction which is offered in the case of buildings.

These specifications are followed by all of the prominent manufacturers and installers of lightning rods, and compliance with them is necessary to secure the approval of Underwriters' Laboratories, Inc. The work of developing them was done in coöperation with a committee of the National Fire Protection Association, and the standard of this organization makes the same requirements.

The question of how much protection to a building is afforded by lightning rods is often asked. It is admitted that this protection is not perfect if the requirements of the code alone are followed. An elaborate metallic cage about the building would be necessary. However, compliance with the code does give a high degree of protection, as is evidenced by the reports of many State fire marshals on fires which are caused by lightning. Nearly all cases of loss are for buildings which have not been rodded, and in the few cases of loss of rodded buildings there is a question whether the installation has been kept in good condition. For instance, conductors sometimes corrode at the ground line, thus breaking the connection. The chances of injury to a building which is properly rodded are very small. This does not mean that rodded buildings are not struck by lightning, but that, when struck the discharge is carried harmlessly to ground. Since losses by fire from lightning have been

greatly reduced by the installation of lightning rods, these are especially recommended for isolated farm buildings where the likelihood of damage is much greater than in a city.

In spite of the value of lightning rods, their use has at times and in some places come into disrepute owing to a general lack of information on the best methods of protection. It is hoped that the publication of definite standards will enable the public to demand installations which will give them adequate protection.

Further experience and the accumulating knowledge about lightning may lead later to changes in these specifications, and revisions will be made from time to time as experience and progress in the art may warrant. Comments on these specifications and recommended changes are invited by the committee from all who have had experience in applying them.

SPECTRUM OF LUTECIUM MONOXIDE.

Atoms are characterized by line spectra, but so-called band spectra are always associated with chemical compounds. Band spectra ascribed to monoxides of a large number of the elements are known but, up to the present, the emission spectrum of only one rare-earth (gadolinium) monoxide has been reported. This was observed in the oxyhydrogen flame and was said to vanish at arc temperature. Most of the monoxide compounds which have revealed spectra are chemically unstable, and instability at arc temperature may account for the general lack of data on the spectra of rare-earth oxides. Among the 14 rare earths, the heaviest, lutecium, is unique in showing a sequence of intense blue bands in the conventional arc spectrum. This sequence and 2 parallel sequences of faint bands form a band system characteristic of lutecium monoxide molecules, as shown by a vibrational analysis conducted by William W. Watson and William F. Meggers, and reported in the *Journal of Research* for February (RP1071).

SOLUBILITY OF GLASS AND GLASS ELECTRODE DEPARTURES.

Applying the method for determining the solubility of optical glasses, described in Technical News Bulletin 221

(September, 1935) to a glass which is commonly used for making glass electrodes, the following results were obtained by E. H. Hamilton, Donald Hubbard, and A. N. Finn:

1. The solubility increases rapidly for most alkaline solutions as the pH increases above 8.5 or 9.

2. There is a slight decrease in solubility as the pH changes from 8.5 to 2, but between these concentrations the solubility is much less than for alkalinities above pH 8.5.

3. The solubility decreases rapidly in the acid range beyond pH 2.

Considering these statements in connection with the performance of glass electrodes, these regions of marked solubility change (pH 8.5 and 2) correspond exactly to the two regions of pronounced voltage departures exhibited by electrodes made from this glass.

That the voltage anomalies of the glass electrode are definitely associated with the solubility of the glass has been further demonstrated by the use of glasses which do not have the marked solubility change in the "super acid" region, and by measurements made in alkaline solutions in which soluble silicates are not formed, such as aqueous solutions of ammonia. Under these conditions the voltage departures are very slight.

USE OF STANDARD DYEINGS IN TESTS FOR COLOR-FASTNESS TO LIGHT.

A paper by W. D. Appel, chief of the Bureau's textile section, which will be published soon in the *American Dyestuff Reporter*, deals with the testing and rating of textiles with respect to color-fastness to light. It has a particular significance at this time, as a result of the recent action of the Research Committee of the American Association of Textile Chemists and Colorists, which has adopted standard dyeings of known fastness and is undertaking to make them available to all laboratories. These dyeings will enable the laboratories for the first time to express fastness on a common basis.

The standards are four in number and represent the minimum requirements for fastness classes 1, 3, 5, and 8

(in increasing order of fastness). The same standards have been adopted by the British and the German fastness committees and may be regarded as international. The selection of the intermediate standards, 2, 4, 6, and 7 ,which are needed principally for classifying dyestuffs, is being held in abeyance pending further discussion by the committees abroad.

Why such standards are necessary and how they can be used to the best advantage are discussed in the paper.

The ideal test for color-fastness to light would require apparatus with which the fading resulting from exposure in service could be duplicated in a short time (say 24 hours), and instrumental means for measuring the fading. The first condition is approximated in available apparatus, but the amount of fading produced by exposure for a given number of hours both in the same and in different fading lamps varies greatly from time to time. The cost of the equipment to eliminate this variation is prohibitive. Fortunately, the relative fading of dyeings in these lamps is reproducible. By testing simultaneously the fastness of the materials in question and of standard dyeings of known fastness issued to all laboratories by a central source, it will be possible for all to arrive at the same ratings of fastness.

Quantitative methods for evaluating the changes in color of dyeings when they fade are not available in most textile laboratories and they are too slow for routine testing, though they may be used to advantage when visual judgment is uncertain. The changes in color of the standard dyeings furnish a guide when the method of visual inspection is used. Most fading can be judged rapidly and satisfactorily in this way.

CREASE-RESISTANT FINISHES FOR FABRICS.

With the advent of crease-resistant finishes in the commercial production of cloths it has become increasingly important to measure the improvement of the cloths resulting from the finishing treatment and also the resilience of fabrics in general. In the *Journal of Research* for February (RP1077), Herbert F. Schiefer reports on work undertaken to determine the flexural characteristics of representative woven fabrics before and after the application of commercial

crease-resistant finishes using three different methods: Creasing, angle, flexometer, and modified compressometer. Specimens of paper, cellophane, cotton, rayon, worsted, and rubber, which are recognized to vary in resistance to creasing over a considerable range, were also measured, and the results used for comparison.

The crease-resistant finishing treatments increased the energy required to deform the cloth specimens, that is, increased the stiffness of the cloths. This increase exceeded 100 per cent. for some of the cloths. The energy of recovery was also increased by these treatments, the increase ranging from 20 to 100 per cent. Neither of these two quantities can be taken individually as a measure of crease resistance. However, the ratio of the latter to the former, is related to the resistance to creasing. The resilience of nearly all of the cloths, as determined by each of the three test methods, was increased by the crease-resistant finishing treatments.

The three test methods described can be used to evaluate the improvement given by crease-resistant finishes, using the measurements on the cloths before treatment as the basis of comparison. They should be valuable in systematic studies of the effect of different finishing treatments and in determining the effects of relative humidity, temperature, and other factors on the different finishes. Method no. 1, however, does not give a critical measure of stiffness, a property that is greatly affected by crease-resistant finishes and should be measured. The resilience as determined by these methods is dependent upon the testing conditions, as for example the magnitude of the maximum load in method no. 3. It may be found necessary in test methods of this type to vary the maximum load with the thickness or weight of the fabric to be tested.

WEATHERING TESTS ON FILLED COATING ASPHALTS.

Asphalt shingles and roll roofings are made by impregnating felt with a relatively soft asphalt and then surfacing with a harder asphalt known as a coating asphalt. In recent years it has become general practice to mix finely ground slate, limestone, and similar mineral fillers with the asphalt coatings.

However, as data were lacking to show the serviceability of filled coatings, an investigation was undertaken by O. G. Strieter, research associate of the Asphalt Shingle and Roofing Institute, to determine the effect of various kinds and grades of commercial fillers on the weather-resisting properties of coating asphalt. A variety of asphalt-filler mixtures were prepared and their weather-resisting properties were determined by exposing them outdoors in various localities, and to the accelerated weathering test.

These weathering tests, outdoor and accelerated, are described in detail in RP1073, in the *Journal of Research* for February. The tests showed that suitable fillers greatly increase the life of an asphalt, although all the fillers tested did not prove equally effective.

FIRE TESTS OF TREATED AND UNTREATED WOOD PARTITIONS.

Fire-protection engineers now use a standard fire-testing procedure to determine how well a wall, partition, or door will resist destruction and heat transmission when subjected to fire. Recently Clement R. Brown of the Bureau's fire resistance section conducted some fire tests on thirteen 4 ft. square partitions and four larger partitions built of wood. Some of the small partitions were built of untreated wood, but most of them were constructed of wood treated with fire-retarding chemicals. The panels differed in thickness, design, and amount of treatment. The results of these tests are given in RP1076 which will be published in the *Journal of Research* for February. It was found that, as measured by these tests, the resistance to penetration of fire and heat through the partition was affected little by treatment, but appreciably by thickness and design. The principal value of treatment was to reduce the amount of flaming during and, more particularly, after the fire test.

As compared with fire-tube and flame-penetration tests on small specimens representative of each panel, the results show that only the flame-penetration test is related to the standard fire test, and in this case, only to a limited extent.

WATER PENETRABILITY OF CLAY AND SHALE BUILDING BRICK.

Upper and lower limits for water absorption were included in the early specifications for building bricks. Since the methods of test for water absoprtion involve the total immersion of the test specimen for a relatively long period (usually 5 hours or more), this test is a measure of the absorptive capacity of the brick. Other tests have been devised which measure the rapidity with which water is absorbed, among such tests being the determination of gain in weight of the brick resulting from partial immersion in water for 1 or 3 minutes, and the measurement of the time required for the complete absorption of 1 ml of water spread over 5 sq. cm. of the brick surface. Since these short time tests measure the ease of penetration of water into brick, they are called penetrability tests.

The penetrability of bricks has been shown to have a great effect on the strength and quality of bond between brick and mortar. Specifications have been written which set upper and lower limits on penetrability. Obviously the utility of such a specification will depend in part upon the possibility of securing commercial supplies of brick which conform to it. A study of this subject has been made at the Bureau and a paper entitled "Penetrability of Clay and Shale Building Brick," by J. W. McBurney and A. R. Eberle, has been submitted for publication in the *Journal of the American Ceramic Society*. Results are presented from tests of approximately 1,600 bricks which were a random selection from a sample of 3,886 bricks representing the product of about 250 plants in the United states. The effect on penetrability of raw material (clay or shale), method of forming (dry press, soft mud, or stiff mud) and geographical location, was studied. Comparisons of results of tests by several methods are included. The principal conclusions are:

(1) The relation between gain in weight by 1-minute and 3-minute partial immersion in water is lineal for bricks which do not wet through in 3 minutes under the conditions of this test.

(2) The average gain in weight by 1-minute partial immersion is 58 per cent. of the average gain in weight by 3-minutes partial immersion according to these data.

(3) Considering all raw materials and methods of forming, the relation between the average gain in weight by partial immersion in water for 3 minutes and average water absorption (total immersion at room temperature for 5 hours, or 48 hours, or by boiling for 5 hours) is fairly regular up to 44 grams (approximately 1.5 ounces) gain in weight by partial immersion for 3 minutes. The relation becomes increasingly erratic as the gain in weight by 3-minutes partial immersion increases.

(4) Fifty-eight per cent. of the shale bricks had gains in weight by 3-minute partial immersion of less than one ounce; the corresponding figure for the clay bricks was 36 per cent. The range 1 to 4 ounces included 36 per cent. of the shale bricks and 55 per cent. of the clay bricks.

(5) Bricks formed by the dry press process have notably greater penetrabilities than those formed by the soft-mud, stiff-mud side-cut, or stiff-mud end-cut processes.

(6) In only 12 of 23 districts did 50 per cent. or more of the specimens fall within the limits of 1 to 4 ounces gain in weight by partial immersion in water for 3 minutes. Three districts (Hudson Valley, Philadelphia and Chicago) provided specimens of which more than 75 per cent. fell within this limit.

Science and Living.—DR. ALFRED N. GOLDSMITH, technical consultant to the Radio Corporation of America, told the convention of New York State Science Teachers recently, in the course of an address on television, "The approach of television as a home service emphasizes the rapidly growing influence of science upon our existence, and the time has arrived when everyone should understand at least the fundamental principles of the many technical devices in daily use. Unless we understand the natural laws of this world and the rules which govern our implements and surroundings, we can hardly hope to live effectively and comfortably. Ignorance of these physical laws means continual blunders, misuse of our devices and unpleasant collision with facts. On the other hand, an appreciation of them results in fuller comprehension of the world about us and a more effective use of technical equipment available for our convenience. . . . It is strange that while we appropriately inculate appreciation for history and the classical arts, so little time is allotted for the study of the realm of reality in which we live."

R. H. O.

THE FRANKLIN INSTITUTE.

STATED MONTHLY MEETING, FEBRUARY 16, 1938.

The regular monthly meeting of The Franklin Institute was called to order at 8:30 o'clock by Mr. W. Chattin Wetherill, Vice-President. He announced that the minutes of the previous meeting had been printed in the February issue of THE JOURNAL of The Institute and, if there were no objections or corrections, the minutes would be approved. Upon hearing none, the Chairman declared them approved as printed.

The additions to membership since the last report were as follows:

Active	12
Associate	15
Student	37
Total	64

It was also announced that the complete list of Standing Committees appointed on February 2, would appear in full in the March issue of THE JOURNAL.

The Board of Managers recommended the election of the following gentlemen, recipients of the Franklin Medal, for Honorary Membership:

Dr. William Frederick Durand, Leland Stanford Jr. University, California.

Dr. Charles August Kraus, Brown University, Providence, R. I.

No objection being offered, it was so ordered.

The Chairman then introduced Dr. Robert J. Van de Graaff, Associate Professor of Physics, Massachusetts Institute of Technology, Cambridge, Mass., who spoke on "Electrostatic High Voltage Generators."

An account was given of the development of electrostatic generators from the time of Otto von Guericke with later developments by Newton and Franklin. He described generators of the belt type and their application to vacuum tubes for the production of high-energy radiations. This type of generator consists essentially of a rounded conducting terminal supported on an insulating column and charged by a swiftly moving belt, conveying electricity from earth potential and depositing it within the terminal. Potentials of several million volts have been obtained, the only limit being breakdown of the insulation provided for the high voltage electrode. The use of compressed gas insulation was discussed together with the gains in power and compactness which may be thus attained. Mention was also made of the application of the high-energy radiations to the investigation of atomic disintegration and also to the treatment of cancer. The subject was illustrated by numerous lantern slides.

A discussion followed the reading of the paper, after which the Chairman conveyed the thanks of those present to the speaker.

Adjourned.

HENRY BUTLER ALLEN,
Secretary.

LIBRARY NOTES.

The Committee on Library desires to add to the collections of the Institute any technical writings of members who have had occasion to publish such material. Literary contributions from author-members will be gratefully acknowledged, properly inscribed and noted in the Journal of the Institute.

Photostat Service. Photostat prints of any material in the collections can be supplied on request. Orders received in the morning are filled the same day. The average cost for a print $8\frac{1}{2} \times 11$ inches is thirty-five cents.

The library and reading room are open Mondays, Tuesdays, Fridays and Saturdays from nine o'clock A.M. until five o'clock P.M., Wednesdays and Thursdays two until ten o'clock P.M., 775 readers made use of the facilities during the twenty-five days of January.

RECENT ADDITIONS.

Astronomy.

FOCARD, JACQUES. Paraphrase de l'Astrolabe; contenant: Les Principes de Geometrie; La Sphere; l'Astrolabe, ou, declaracion des choses celestes; Le Miroir du Monde, ou, exposicion des parties de la Terre. Revue et corrigee par Jacques Bassentin, Escossois, avec une Amplificacion de l'usage de l'Astrolabe par luimesme ajoutee. 1555.

MARTIN, JOH. De Usu Astrolabi; Compendium, Schematibus commodissimis illustratum, ac mendis quampluribus repurgatum. 1553.

MERRILL, PAUL W. The Nature of Variable Stars. 1938.

STOEFFLER, JOHN. Tratado de el Astrolabio, en que se Explica su Fabrica y Construccion y sus muchos y admirables usos. Su Autor Juan Stoflexino Justingense. 1570.

Automotive Engineering.

SAUNIER, BAUDRY DE, CHARLES DOLLFUS, AND EDGAR DE GEOFFROY. Histoire de la Locomotion Terrestre. La Locomotion Naturelle, L'Attelage, La Voiture, le Cyclisme, la Locomotion Mécanique, l'Automobile. 1936.

Aviation.

DOLLFUS, CHARLES, AND HENRI BOUCHÉ. Histoire de l'Aéronautique, 1932.

SPAREY, LAWRENCE H., AND CHARLES A. RIPPON. The New Model Aeroplane Manual. A Practical Handbook on the Building and Flying of Model Aeroplanes. No date. Marshall's Practical Manuals No. 12.

Biology and Biochemistry.

MACCURDY, GEORGE GRANT, Editor. Early Man as Depicted by Leading Authorities at the International Symposium, The Academy of Natural Sciences, Philadelphia, March 1937.

SOBOTKA, HARRY. Physiological Chemistry of the Bile. 1937.

Chemistry and Chemical Technology.

Analyst. Decennial Index. Volumes 51–60, 1926–1935.

Beilsteins Handbuch der organischen Chemie. Vierte Auflage. Siebenundzwanzigster Band. 1937.

Directories.

Thomas' Register of American Manufacturers 1938.

Who's Who in Railroading. United States—Canada—Mexico—Cuba. 1930 Edition.

Economics.

REICH, EDWARD, AND CARLTON JOHN SIEGLER. Consumer Goods How to Know and Use Them. 1937.

Electricity and Electrical Engineering.

BARKHAUSEN, H. Les Tubes à Vide et leurs Applications. Traduit de l'Allemand par Ed. Labin. Tome 2: Les Amplificateurs. 1937.

MCSPADDEN, J. WALKER. How They Sent the News. 1937.

World Power Conference. Statistical Year Book, 1934–1935. Volume 2. 1937.

Engineering.

American Society for Testing Materials. Proceedings of the Fortieth Annual Meeting. 1937. Volume 37, Parts 1–2. 1938.

PIPPARD, A. J. S., AND J. F. BAKER. The Analysis of Engineering Structures. 1936.

Geology.

JOHANNSEN, ALBERT. Manual of Petrographic Methods. Second Edition. 1918

Mathematics.

SMOLEY, C. K. Segmental Functions Text and Tables. 1937.

Mechanical Engineering.

KLEMGARD, E. N. Lubricating Greases: their Manufacture and Use. 1937.

Mining Engineering.

American Institute of Mining and Metallurgical Engineers. Transactions, Iron and Steel Division. 1937.

Naval Architecture and Navigation.

TOUDOUZE, GEORGES G., AND OTHERS. Histoire de la Marine. 1934.

Photography and Printing.

Klimschs Jahrbuch des graphischen Gewerbes. 31 Band. 1938.

Penrose Annual. Volume 40. 1938.

Photograms of the Year 1938. 43d Year of Publication.

Railroad Engineering.

DOLLFUS, CHARLES, AND EDGAR DE GEOFFROY. Histoire de la Locomotion Terrestre: Les Chemins de Fer. 1935.

Scientific Essays.

DUNNE, J. W. An Experiment with Time. 2nd edition. 1937.

Royal Society of Edinburgh. Transactions 1936–37. Volume 59 Part 1. 1937.

Statistics.

Bulletin Almanac and Year Book. 1938.

World Almanac and Book of Facts for 1938.

Sugar.

Annuaire Sucrier. Soixante-Troisième Année. 1937.

BOOK REVIEWS

CONSUMER GOODS, HOW TO KNOW AND USE THEM, by Edward Reich and Carlton John Siegler. 526 pages, illustrations, 15 × 22 cms. New York, American Book Company, 1937.

Probably everyone who buys an article for his or her own use feels at one time or another a need for a method of determining the value of the prospective purchase. Large industrial concerns and even the Government itself have buyers trained in the specialized class of materials which they purchase and when there is the least doubt as to the quality of the goods to be purchased, whether they will satisfy the specific desires of the purchasor, laboratories are engaged to conduct tests and issue specifications. The individual, however, buys a great variety of materials for his own consumption without the benefit of specialized knowledge of the materials. The result of this ignorance is often costly. And further, an understanding of goods makes for better use of the materials purchased and consequently a more efficient consumer.

Recognizing this need, many schools have developed courses in consumer training. They are known as home-economics courses, homemaking courses, consumer courses, textile courses, etc. The book at hand is a text for a consumer course. It has direct relationship with commercial subjects and with home economics, and it offers functional correlation with the social and natural sciences. It is an unusual book for two reasons: (1) because it covers a rather broad field; and (2) because in the method of presenting essential facts about consumer goods it draws on geography, history, civics, physics, chemistry and related subjects.

Descriptions are given of a great many of the materials that the everyday consumer is interested in such as cotton, wool, linen, silk, rayon, fur, leather, wood, paper, rubber, glass, chinaware, precious metals, gems, oils, paints and varnishes, cosmetics, and foods. A study is made of the origin of these goods, their processes of manufacture, the different types in use, and the applications to which they are put. Methods are explained for judging and testing the genuineness of the goods and there are hints for their proper care. The history, the geographical location, and the economic and social significance of each article are also presented.

The approach to each subject is done in such a way that interest is stimulated and the presentation is simple and straightforward without going outside of the knowledge limitations of the average individual. The book is well illustrated with pictures, charts and tables. At the end of each chapter there are questions on the text, a glossary of terms used, interesting projects, and a reading list of books related to the subject. There is a subject index at the end.

Those who are interested in more economical, efficient, and appreciative living will do well to become acquainted with this book for study as well as for reference.

R. H. OPPERMANN.

SEGMENTAL FUNCTIONS, TEXT AND TABLES, by C. K. Smoley. 435 pages, 12 × 18 cms. Scranton, C. K. Smoley and Sons, 1937. Price $5.00.

There is considerable need among many engineers and architects for data which can be used for the solution of problems in circular segments. The author of this little book endeavors to satisfy that need. A general idea of the contents

may be had by first calling to mind the solution of triangles. In a triangle there are six parts—three sides and three angles. When three parts, of which at least one is a side, are known, the trigonometric functions offer a means of computing the other three unknown parts. Likewise, in a segment there are essentially five parts—the arc, the chord, the radius, the central angle subtended by the arc, and the height or the middle ordinate referred to the chord as abscissa. When any two parts are known, the other three, and also the area, may be determined by means of the segmental functions introduced in this volume. But since work of that kind is simplified and shortened by the use of logarithms, the author preferred to use the logarithms of the segmental functions instead of the functions themselves and has evolved a set of formulas which are well adapted for logarithmic work, so that the solutions are handled by means of additions and subtractions. The tables in this volume are divided into two groups. Group 1 contains a table of logarithms of the seven segmental functions for angles from 0 to 180 deg. and others which take care of corrections for seconds. Also in this group is a table which gives the natural value of function A, and a table containing logarithms of segments for radius 1.

In Group 2 there is a table of logarithms of numbers from 0 to 10,000, the decimal as well as the English system of linear measure extended to 200 feet. The book should come in very handy to those whose work requires such computations.

R. H. OPPERMANN.

NATIONAL RESEARCH COUNCIL: Transactions of the American Geophysical Union, Eighteenth Annual Meeting, April 28, 29, 30, 1937, Washington, D. C. Part I, 264 pages, illustrations, tables. Part II, 397 pages, illustrations, tables. 17 × 25 cms. Washington, National Research Council, 1937.

This volume, in two parts, is a miscellaneous publication of the Council. It includes the transactions of the eighteenth annual meeting held in Washington, D. C., and Denver, Colo., of the American Geophysical Union. The first part contains reports and papers presented before the general assembly and sections of geodesy, seismology, meteorology, terrestrial magnetism and electricity, oceanography, and volcanology. Part two covers the section of hydrology and South Continental Divide Snow-Survey Conference.

R. H. OPPERMANN.

NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS.

Report No. 614, Pressure Distribution over an N.A.C.A. 23012 Airfoil with an N.A.C.A. 23012 External-Airfoil Flap, by Carl J. Wenzinger. 15 pages, illustrations, 23 × 29 cms. Washington, Government Printing Office, 1938. Price 10 cents.

Pressure-distribution tests of an N.A.C.A. 23012 airfoil with an N.A.C.A. 23012 external-airfoil flap were made in the 7- by 10-foot wind tunnel. The pressures were measured on the upper and lower surfaces at one chord section on both the main airfoil and on the flap for several different flap deflections and at several angles of attack. A test installation was used in which the airfoil was mounted horizontally in the wind tunnel between vertical end planes so that two-dimensional flow was approximated.

The data are presented in the form of pressure-distribution diagrams and as

graphs of calculated coefficients for the airfoil-and-flap combination and for the flap alone. The pressure-distribution tests showed that, as with other types of flap, the greater part of the increment of total maximum lift due to deflecting the external-airfoil flap downward arises from the increased load carried by the main airfoil The maximum normal-force coefficient of the external-airfoil flap was about the same as that of a split flap. The hinge moments, however, were much lower because of the axis location used with the external-airfoil flap. The pressure diagrams showed that, when the plain airfoil and the flapped airfoil are compared at the same total lift, the flap reduces the adverse pressure gradients and the tendency of the main airfoil to stall. When the plain and flapped airfoils are compared at the same angle of attack, it is apparent that the flap influences the air flow around the main airfoil so that the airfoil carries a much greater load without stalling than is possible without the flap.

No. 616, Interrelation of Exhaust-Gas Constitutents, by Harold C. Gerrish and Fred Voss. 8 pages, illustrations, 23 × 29 cms. Washington, Government Printing Office, 1937. Price 10 cents.

An investigation was made to determine the interrelation of the constituents of the exhaust gases of internal-combustion engines and the effect of engine performance on these relations. Six single-cylinder, liquid-cooled test engines and one 9-cylinder radial air-cooled engine were tested. Various types of combustion chambers were used and the engines were operated at compression ratios from 5.1 to 7.0 using spark ignition and from 13.5 to 15.6 using compression ignition. The investigation covered a range of engine speeds from 1,500 to 2,100 r.p.m. The fuels used were two grades of aviation gasoline, Auto Diesel fuel, and Laboratory Diesel fuel. Power, friction, and fuel-consumption data were obtained from the single-cylinder engines at the same time that the exhaust-gas samples were collected.

Definite relations, which were independent of engine design and operating conditions, were found among the constitutents of exhaust gases, air-fuel ratio, water of combustion, and combustion efficiency. Combustion efficiency and amount of water of combustion increased approximately linearly with air-fuel ratio for rich mixtures and were independent of mixture strength for lean mixtures. These relations make it possible to obtain a complete exhaust-gas analysis simply by determining the air-fuel ratio or the CO_2 and O_2 content. The results also showed that compression-ignition engines may be operated at the same air-fuel ratio as spark-ignition engines without loss in combustion efficiency.

ASSOCIATION OF CONSULTING CHEMISTS AND CHEMICAL ENGINEERS, INC. DIRECTORY OF ASSOCIATION MEMBERS, JANUARY 1, 1938. Fourth edition. 22 × 28 cms., variously paged. New York, Association, 1938.

A new departure in membership directories. After a foreword and excerpts from the By-Laws, five pages are devoted to an alphabetical list of the members, this is followed by a list of members in numerical order of members' certificates. A geographical list is next in order and then a list of organizations and firms with which the members are identified. One page is devoted to a detailed account of the officers, staff, scope and activities of each laboratory. There is a key sheet

of members and another, covering eight pages, giving a list of the subjects in which the several members are most qualified to give advice and help.

It is a new kind of directory and should be of service and interest to all who are in touch with the chemical industries.

R.

THE CHEMISTRY OF PETROLEUM DERIVATIVES, by Carleton Ellis. Volume II, 1464 pages, illustrations, 16 × 23 cms. New York, Reinhold Publishing Corporation, 1937. Price $20.00.

It is probably safe to say that everyone interested in the technical phase of petroleum derivatives is familiar with volume one of this set of books which was published in 1934. It is a monumental work covering a vast field. So much has been done in the rather short time since then in the development, investigation, processing, and enlargement of this subject that the author herewith presents volume two as a supplement or continuation of the first volume. It therefore brings the treatment up to date. As the title implies it is not restricted to petroleum chemistry but covers also the ever broadening domain of allied researches. In order to do this adequately there is required considerable space; consequently the book contains 1464 pages divided into fifty four chapters. The text draws from some 6000 references to work recorded in all kinds of technical literature.

Among the most important material to the petroleum chemist to which attention has been directed since 1934 and which is recorded in this book is the subject of sulphur and sulphonation, dealing with the utilization of by-products of refining; polymerization, in the synthesis of motor fuels from gaseous hydrocarbons resulting from cracking or distillation operations; detonation; and nitration.

The book is larger than the first volume by four chapters which have been added because of the increasing interest in these fields. They are on petroleum asphalts, the synthesis of hydrocarbons from carbon monoxide and hydrogen, petroleum hydrocarbons in anesthesia, and thermodynamics and its applications to petroleum. The treatment on asphalts is limited to the preparation, properties and uses of those materials obtained in the refining of various petroleum distillates or residues, or by treatment as for example by oxidation.

Reduction of carbon monoxide to hydrocarbons is a subject in this book devoted expressly to a discussion of the process involving atmospheric pressures. The products resulting have considerable bearing on the petroleum industry. Petroleum hydrocarbons in anesthesia is an interesting coverage on ethylene, propene, acetylene, cyclopropane, and their effects. The application of thermodynamic principles to the field of petroleum technology is a comparatively recent addition. The value of numerous thermodynamic concepts, such as enthalpy, entropy, fugacity and free energy, is rapidly becoming recognized not only in scientific investigations but also in commercial enterprises. Therefore the general discussion given here in regard to the practical application of these topics should be of much interest.

The book contains some 350 photographs and drawings exemplifying the important phases of the art. The very nature of the book requires a comprehensive name and subject index and these are supplied in excellent style.

It is a book of information for study and reference and should have wide application in the petroleum and allied fields.

R. H. OPPERMANN.

LIGHT, PHOTOMETRY AND ILLUMINATING ENGINEERING, by William E. Barrows. Second Edition, 445 pages, illustrations, 15×23 cms. New York, McGraw-Hill Book Company, Inc., 1938. Price $4.00.

The progress of illuminating engineering is plainly evident on all sides and that it is destined for a great future is a foregone conclusion. The profession is one that requires considerable basic and important knowledge, therefore the selection of a text should be made with utmost care. It should contain all of the fundamentals upon which present day practice leans as well as what may be the foundation of future illumination. A thorough grounding in this field is certainly worth while for those who intend to follow it.

The author of this book is well known for his previous works. He has presented here the second edition of his book (the first being dated 1925). It is designed as a text for students as well as a reference book for practicing engineers. As such, the treatment progresses from the simple to the more complex. Light and vision is first taken up describing the nature of light both natural and artificial, the construction and characteristics of the human eye and the effects of color, intensities and reflected surfaces. Standards of luminosity are then explained together with units and the nomenclature. After discussing generally the sources of light the characteristics of various gas and vapor lamps are investigated and the subject of radiation from these lamps. The measurement of luminous intensity is covered by chapters on photometry and types of photometers both in the laboratory and portable instruments for use in the field, which are followed by explanations on light-flux calculations. It is pointed out here that curves plotted to polar coördinates merely show the distribution of light in one plane around the source of light as a center and have no significance as a representation of the quantity of light.

The more practical side of the subject begins with a treatment on principles of interior lighting wherein modern lighting standards are listed and discussed. After this the point-by-point method and the flux-of-light method of predetermining the illumination to be produced by a given lighting installation are described with reasons for and instances in the use of each. Recent innovations are adequately covered such as cove lighting, spot lights, ceiling reflection, exterior lighting, as well as street and highway lighting, and automobile and airway lighting.

The book is written in a style that should make for quick understanding; it is well illustrated, and it contains a useful index.

R. H. OPPERMANN.

PUBLICATIONS RECEIVED.

Milestones in Medicine, Laity Lectures of the New York Academy of Medicine, introduction by James Alexander Miller. 275 pages, illustrations, 13 × 20 cms. New York, D. Appleton-Century Company, 1938. Price $2.00.

The Elements of Quantum Mechanics, by Saul Dushman. 452 pages, illustrations, 15 × 23 cms. New York, John Wiley & Sons, Inc., 1938. Price $5.00.

Edward Wilson of the Antarctic, by George Seaver, with an introduction by Apsley Cherry-Gerrard. 301 pages, plates, 14 × 22 cms. New York, E. P. Dutton & Co., Inc., 1938. Price $3.00.

Light, Photometry and Illuminating Engineering, by William E. Barrows. Second edition, 445 pages, illustrations, 15 × 23 cms. New York, McGraw-Hill Book Company, Inc., 1938. Price $4.00

Low Temperature Physics, by M. and B. Ruhemann. 313 pages, illustrations, 14 × 21 cms. New York, The Macmillan Company, 1937. Price $5.00.

Colloid Systems, a survey of the phenomena of modern colloid physics and chemistry, by Priv.-Doz Dr. A. Von Buzágh, translated by Otto B. Darbishire, edited by William Clayton. 311 pages, illustrations, tables, 16 × 25 cms. London, The Technical Press Ltd., 1937. Price $9.00.

Theorie und Anwendung der Laplace-Transformation, von Gustav Doetsch. 436 pages, illustrations, 15 × 25 cms. Berlin, Verlag von Julius Springer, 1937. Price 34.50 R.M.

Biography of Benjamin Smith Lyman, by Gonpei Kuwada. 104 pages, plates, 13 × 20 cms. Supplement, Shojiki Shokichi, The Honest Rikisha Man. 10 pages, 13 × 20 cms. Toyky, Sanseido Co., Ltd., 1937.

Methoden Der Mathematischen Physik, by R. Courant and D. Hilbert. Zweiter Band. 549 pages, illustrations, 16 × 24 cms. Berlin, Verlag von Julius Springer, 1937. Price 38 R.M.

U. S. Department of Commerce, National Bureau of Standards. Circular C418, Architectural Acoustics, by Paul R. Heyl and V. L. Chrisler. 11 pages, illustrations. Price 5 cents. Research Paper RP1048, Study of Vibrated Concrete, by John Tucker, Jr., G. L. Pigman, E. A. Pisapia, and J. S. Rogers. 17 pages, illustrations. Research Paper RP1051, Dimensional Changes in Aerial Photographic Films and Papers, by Raymond Davis and Emory J. Stovall, Jr. 23 pages, illustrations. Simplified Practice Recommendation R169-37 for Machine, Carriage and Lag Bolts (Steel). 21 pages, illustrations. Price 5 cents. 4 pamphlets, 15 × 23 cms. Washington, Superintendent of Documents, 1937.

Bell Telephone System, Monographs: B-1026, Current Amplitude and Phase Relations in Antenna Arrays, by John F. Morrison. 17 pages, illustrations. B-1027, A Negative-Grid Triode Oscillator and Amplifier for Ultra-High Frequencies, by A. L. Samuel. 10 pages, illustrations. B-1029, Transmission Theory of Plane Electromagnetic Waves, by S. A. Schelkunoff. 36 pages, illustrations. B-1030, On the Ionization of the F_2 Region, by W. M. Goodall. 5 pages, illustrations. B-1031, Transmitted Frequency Range for Circuits in Broad-Band Systems, by H. A. Affel. 6 pages, illustrations. B-1032, Variable Frequency Electric Circuit Theory, by John R. Carson and Thornton C. Fry. 28 pages, illustrations. B-1034, Irregularities in Broad-Band Wire Transmission Circuits, by Pierre Mertz and K. W. Pfleger. 19 pages, illustrations. B-1035, The Dielectric Properties of Insulating Materials, by E. J. Murphy and S. O. Morgan. 20 pages, illustrations. B-1036, Magnetic Generation of a Group of Harmonics, by E. Peterson, J. M. Manley and L. R. Wrathall. 19 pages, illustrations. B-1037, Radio Telephone Noise Reduction by Voice Control at Receiver, by C. C. Taylor. 12 pages, illustrations. B-1038, The Vodas, by S. B. Wright. 19 pages, illustrations. B-1039, Extending the Frequency Range of the Negative Grid Tube, by A. L. Samuel. 32 pages, illustrations. B-1040, Energy of Lattice Distortion in Cold Worked Permalloy, by F. E. Haworth. 15 pages, illustrations. B-1041, Minimum Noise Levels Obtained on Short-Wave Radio Receiving Systems, by Karl G. Jansky. 14 pages, illustrations. B-1042, Electron Diffrac-

tion Studies of Cuprous Oxide, by L. H. Germer. 22 pages, illustrations. 15 pamphlets, 15 × 23 cms. New York, Bell Laboratories, 1937.

U. S. Department of Commerce. Aids to Navigation on the Coasts and Waterways of the United States, Statistics, June 30, 1937. 13 pages. Lighthouse Service. 13 pages. 2 pamphlets, 15 × 23 cms. Washington, Government Printing Office, 1938.

National Advisory Committee for Aeronautics, Technical Notes: No. 631, Wind-Tunnel Tests of Carburetor-Intake Rams, by Frank H. Highley. 18 pages, illustrations. No 632, Improvement of Aileron Effectiveness by the Prevention of Air Leakage Through the Hinge Gas as Determined in Flight, by H. A. Soulé and W. Gracey. 17 pages, illustrations. No. 633, Spinning Characteristics of Wings, V- N.A.C.A. 0009, and 6718 Monoplane Wings, by M. J. Bamber and R. O. House. 34 pages, illustrations. No. 634, The N.A.C.A. Optical Engine Indicator, by Robert E. Tozier. 12 pages, illustrations. 4 pamphlets, 20 × 26 cms. Washington, Committee, 1938.

Canada, Department of Trade and Commerce, Dominion Bureau of Statistics, Census of Industry, General Manufactures Branch. Report on the Hosiery, Knitted Goods and Fabric Glove and Mitten Industries in Canada, 1936. 32 pages, 22 × 28 cms. Ottawa, Minister of Trade and Commerce, 1938. Price 25 cents.

Great Britain, Department of Scientific and Industrial Research. Illumination Research Technical Paper No. 20, The Use of Coloured Light for Motor Car Headlights. 32 pages, 15 × 24 cms. New York, British Library of Information, 1937. Price 30 cents.

Planning and Equipping Laboratories for Research. Recent Experience at Mellon Institute, by Harry S. Coleman. 11 pages, illustrations. 20 × 28 cms. Pittsburgh, Mellon Institute of Industrial Research.